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ON THE PSYCHOLOGY OF SO-CALLED PROCESSES OF ABSTRACTION *

BY EGON WEIGL

TRANSLATED BY MARGARET J. RIOCH

EDITED BY CARNEY LANDIS
Columbia University
Psychiatric Institute

AND

KURT GOLDSTEIN
Tufts College
Harvard University

EDITORS' NOTE

In recent years there has been an increase in the interest of American students in the impairment of "abstract behavior" as a phenomenon of psychopathology. This interest is shown by the growing list of publications in which such behavioral change has been studied in cases of brain injury, schizophrenics, aments, children, etc. (1).

The study of impairment of abstract behavior grew out of the work of Gelb and Goldstein, who, during and after the World War, introduced several methods, particularly the sorting-test method, for the analysis of performance changes in patients suffering from brain diseases. The publications on the subject by these authors and their collaborators have not been available in English. One of these German publications that offers an especially instructive experimental analysis of the phenomena and methods in question is this study by Egon Weigl. It seems worth while, therefore, to make available most of the original text of this paper in English so that future investigations may be facilitated.

INTRODUCTION

IN the medical literature there are numerous descriptions of patients with cerebral lesions who show peculiar abnormalities in their behavior with regard to color. If these afflicted persons are presented with a certain color sample (for example, a skein of colored wool, a piece of colored paper, etc.), they are not capable of picking out, in a normal way, lighter or darker shades of the same color to go with the sample, although their sensory ability to distinguish colors is perfectly intact. In experiments with the Holmgren wools, for example, the patients characteristically sort them differently than do normal persons with normal color vision. But, on the other hand, they never make the mistakes which are peculiar to persons without normal color vision. In addition to this abnormal behavior in

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sorting colors, the patients show a number of other pathological symptoms: seldom or never are they able to name correctly colors which are presented to them, and, if the name of a color is given them, they cannot pick out the corresponding shades from a rather large group of samples, etc. We have to do here with a complex symptom, which belongs in the narrow sense to the so-called "amnesia for the names of colors," which in turn is a special case of "amnestic aphasia."

Formerly the various symptoms were usually traced back to separate, co-existing abnormalities. Gelb and Goldstein (2), however, came to the conclusion that all the symptoms can be traced to a unitary, basic abnormality. They were led to this conclusion on the basis of a behavioral analysis, which they made by means of experiments, on both patients and normal persons, concerning the classification of colors with reference to a given sample. In these experiments the main difference in the behavior of the patients and the normal persons can be stated as follows: "In sorting out colors the *attention* of the normal person is forced in a certain *direction* by the instructions given. He looks at the sample solely from the point of view of the simple color regardless of intensity or purity. The concrete color is not appreciated in its purely individual quality, but rather as a representative of the 'concept' red, yellow, blue, etc.; it is appreciated as a *representative* of redness, yellowness, blueness, etc." The authors designate this behavior of normal persons as "categorial behavior." It is only on the basis of this behavior that we can voluntarily select, emphasize, and hold firmly in mind *one* quality in a phenomenon, or in other words, that we can "abstract."

The behavior of patients is distinguished from this rational behavior of normal persons by its greater "primitivity," its "closeness to reality." It appears all the more primitive and less rational, since it is very difficult or even impossible to focus the mental operations of the patients in a particular direction, demanded by the experimenter. Their reactions (for example, in putting skeins together) are determined by the sensorially manifest *color organizations*[1] *and color groupings* which thrust

[1] Translator's note: The German word here is *Gliederung*, which means the organization of a unit consisting of articulated members, as, for example, the body. *Gliederung* will be transated throughout as "organization" (and *gliedern* as "to organize").

themselves upon the patients at the particular moment. Their reaction changes with every change of the color organization and grouping. Consequently, the patients *have no principle of classification,* such as is formed in categorial behavior.

The primitive, concrete behavior of patients was demonstrated in a particularly instructive way in the following experiment. They were given three colors and told to place the two together which were "most alike." Whether or not the desired result was apparently obtained depended on the way the colors were combined. Thus, for example, the patient Th., when presented with a dark red, a pink, and a medium brown skein, reacted in the way intended by the experimenter; that is, he put the two reddish skeins together. If, however, he was given a sky blue skein instead of the medium brown one, the patient usually could not decide at all, because, for him, the paleness of the sky blue and of the pink "cohered" as much as the two variants of the basic color hue: red. Whereas on superficial observation it seems as if the patient, when given the identical task, reacts "correctly" one time and fails another time, the behavioral analysis shows that in reality he does not understand at all what is demanded of him. Since he is not capable of proceeding according to a definite principle of classification, the "similarity" or "dissimilarity" of two colored skeins is determined for him by which two of the three colors are experienced by the patient as belonging together in the most impressive way. Thus in certain presentations the identity of the basic color tone may just as well prevail as the similarity in paleness, and so forth; or two different ways of grouping the three colors of the triad (one according to brightness, the other according to quality) may, so to speak, compete with each other and hold each other in abeyance.

The authors were able to find reactions of this kind even in normal persons, although in less pronounced form, when they behaved completely "passively."

The behavior of patients in the above-mentioned *experiments in the sorting of colors* was quite analogous to that in the particular experiments just described. Hence we are dealing with a pathological change in these patients which is connected with *the problem of so-called "isolating abstraction."* One might

also say (according to Gelb and Goldstein), "The patients did not behave normally with regard to abstraction. To be sure their practical reaction (putting colored skeins together) seems to be determined by particular aspects of the colors presented to them, but this is true only in so far as the practical reaction is determined by definite experiences of coherence,[2] through which and with which certain aspects of color come into prominence. On the other hand, it is impossible or very difficult for such patients to select voluntarily a particular aspect of color, just as it is impossible or difficult to guide their attention in a particular direction. If, in sorting, they put particular colors together, this procedure is forced upon them passively by the experience of coherence at the moment. Furthermore, even when a particular aspect of the sensory impression has forced this procedure upon them, the patients are unable to hold fast to this aspect in their subsequent procedure. They cannot make it into a principle of classification, for the particular aspect of color is fundamentally not present to them at all when isolated in one single skein" (2).

In an appendix Gelb and Goldstein (2) report experiments on the sorting of objects, which they carried out analogously to the color experiments. It thereby became clear "that in amnestic aphasia for objects there is a deep psychic change of the total behavior of the patient, analogous to that in amnesia for the names of colors. The change consists in a descent to a level of behavior which is more primitive and more deeply rooted in living reality" (2). The authors arrived at this hypothesis chiefly through the observation that the sorting or arrangement of objects, as well as of colors, is psychologically something different in patients than it is in normal persons. The patients sort a number of partly heterogeneous, partly homogeneous common objects on the basis of organizations and

[2] Author's note: When in this connection the words "coherence" or "cohering" are used, it does not mean that the particular experience of linking together is "secondary to the qualities of color," in the sense that it is added to certain already existing qualities of color. On the contrary, Gelb and Goldstein (2) are of the opinion that the experience of certain color organizations and groupings is decidedly primary. Similarly Volkelt (3) points out that for children the two figures of like color in the Katz experiments do not "cohere," but rather "form one single configuration, the two members of which are essentially determined by this comprehensive configuration." The "common element, for instance the green, is not selected by the child from two entirely separate objects (the two green figures) by 'attention to only a part of their content,' but it is experienced in the comprehensive configuration, which includes the two members, as the leading, sustaining, and essential aspect."

groupings which they experience concretely and phenomenally; that is, they arrange the objects in the way in which they belong together for them from a palpable, concrete point of view. Here too the organization and grouping are not superimposed by the patient upon the objects to be sorted; but rather he experiences them from the outset as organized and grouped in a definite way. Similarly the homogeneity of the single objects varies with changing organizations and groupings. Although—as we shall see later—this kind of experience is entirely possible for the normal person also, nevertheless the behavior of the normal person differs from that of the patient in that he is at least capable of a different type of procedure. He *can,* to a certain degree, voluntarily superimpose a certain organization upon the given objects.

The problem of the behavior of patients with cerebral lesions and of normal persons in "*sorting*" requires further thorough investigation. In particular it is important to increase, through behavioral analyses, our knowledge of those processes by means of which "sorting according to *various* points of view" is accomplished in patients and, under certain circumstances, in normal persons. Thereby we shall be able to penetrate from a new angle into the problem of the "*processes of abstraction,*" i.e., those processes to which one usually reduces the reaction to the partial conformity of several objects.

I should like at this point to thank Professor Gelb and Professor Goldstein most cordially for the great interest which they have taken in the development of my work and for their assistance and stimulation on numerous occasions.

INVESTIGATIONS CONCERNING THE PROCESS OF SORTING IN PATIENTS WITH CEREBRAL LESIONS, NORMAL ADULTS, AND CHILDREN

The investigations in the first section of this article are predominantly descriptive. We shall choose, from the number of patients with cerebral lesions whom we examined, one with a particularly "typical" manner of reacting, namely L.R., a patient with a frontal lobe injury (4).

The results gained from the other patients and from normal adults and children will be mentioned only for purposes of comparison.

SORTING OF HOLMGREN WOOL SAMPLES

Patient R. was instructed to pick out from a pile of Holmgren wool samples those which are similar to, or go with a particular sample. The patient slowly tried out various skeins by holding the given sample next to each one. In most cases, however, he rejected the skeins singled out and finally decided upon very few—namely the identical ones.[3]

The experimenter now takes a series of different greens out of the pile, puts them together in a bunch and asks the patient whether these don't all belong or fit together. The patient denies this emphatically and says, pointing to a dark skein in the bunch, "Green!" Then he indicates by gestures that the other skeins have nothing to do with this one and that the experimenter should lay them aside. Suddenly, however, he catches sight of a bright green skein in the bunch (which had evidently been concealed from him previously by the others), and now he endeavors vehemently to make clear to the experimenter that he had made a mistake before, that the first-mentioned dark green skein was not green at all; only this one (the bright green) was green. At the same time he searches with great effort for a new designation for the dark green skein. The patient indignantly rejects the suggestion of the experimenter that one might say of this skein too that it is green, that perhaps all the skeins in the bunch are green. The patient continues in his endeavors to find a special name for the dark green skein.

The behavior of this patient conforms strikingly to the types of reaction which Gelb and Goldstein (2) found in their investigations concerning amnestic aphasia. This patient too is not really contented with his choice until he has found the completely identical colors. Similarly he is quite at a loss when confronted with the idea of applying a principle of classification (for example, the category "green"). He does not understand at all what is meant by the statement, "They are all green." The word "green" suits him only when applied to a certain quite definite shade (very glaring). The "mistake" which he believes he has made is characteristic of the primitivity of his behavior. In spite of the fact that he was able to apply the word "green" to both skeins, this occurred in a way which was quite different from what happens with a normal person. Since for him this word is evidently suitable only for a perfectly *definite* color experience, he is, to be sure, capable of designating related experiences by the same term, but only so long as the true shade is not present. As soon as

[3] The color sense of this patient was ascertained to be entirely normal by examination with the Nagel anomaloscope.

he has found *his* nuance, the one first designated is no longer to be considered at all. The procedure of this patient then is "uncategorial" in the highest degree. For him designations fit only a very limited type of experience. How far related impressions can be designated by the same name depends upon how far this type can be stretched. In any case, the patient tends, in so far as possible, to possess a particular expression for each nuance of color tone.

Now that we have seen that the patient is incapable of sorting colors according to conceptually established points of view, it will be of interest to become acquainted with his reaction to objects which the normal person can arrange both according to color and according to form.

SORTING OF CARDBOARD FIGURES OF DIFFERENT COLOR AND FORM

Experiment 1. The patient was presented with 12 unarranged cardboard figures of different color and form: *four equilateral triangles* (red, green, yellow, blue), *four squares* (red, green, yellow, blue), *four circles* (red, green, yellow, blue). He was told to sort them. The patient carried out the task as follows: First he put together a *red* triangle, a *red* circle, and a *red* square in a vertical row. Similarly he put together the yellow, green, and blue figures, so that as a result a series of rows, one red, one yellow, one green, one blue, lay next to each other a few centimeters apart.

The way the patient followed the instructions is extraordinarily typical for the concreteness of his manner of procedure: he "built." Although his procedure was co-determined by the color, he did not form four different irregular piles of color, but he first laid down for himself a pattern out of the red figures, and then copied this in the other colors. The patient could give no explanation of the point of view according to which he undertook the arrangement. When told to sort the figures "differently," the patient proceeded in the following manner: After long consideration he exchanged the circles and triangles within the rows of like color. This procedure shows that the patient is strongly confined in a very definite attitude from which he cannot free himself deliberately. Further, he is not at all capable of reflecting on his method of procedure. These facts strongly indicate that he did not put the figures of like color together because he started from a conceptually fixed principle of sorting, *but rather that his reaction was forced upon him by the sensorially manifest aspects of the situation,*

namely by the element of color and by esthetic factors (symmetry, etc.).

Parallel experiments on normal persons. Let us compare with the above the results obtained from normal adults. The subjects without exception reported that in looking over the given material their first impression was that of "color variety" in which the colors of similar quality seemed to join together in "color spots." Also usually *one* color (red or yellow) thrust itself particularly upon their attention. When told to sort the figures very quickly, inhibiting reflection in so far as possible, most of the subjects—in characteristic contrast to the patient—formed four irregular piles (corresponding to the number of color tones). Only one subject put the figures of like color together in definite patterns, as did the patient. He too "*built*"—from which one sees that the behavior of normal persons, when reacting without any particular reflection, may conform strikingly to that of the patient. The difference between the normal person and the patient becomes clear, however, as soon as the normal person is asked whether one might also sort the figures "differently." The normal subject, without long consideration or delay, then mentions sorting *according to form* as a second possibility.

Experiment 2. In order to attain in the patient as well as in the normal subject an orientation toward form, we presented the figures to him again, unarranged, and repeated the instruction to sort them "differently." After about five minutes the patient decided on an arrangement in which the yellow, blue, green, and red circles were placed on top of each other in an overlapping fashion. He supported it by pointing to each circle and saying, "all different" (whereby he wanted to indicate the difference in the colors). The patient answered in the affirmative to the experimenter's question as to whether one could also have arranged them in an overlapping line of a blue circle, yellow triangle, green circle, and red circle, i.e., substituting a yellow triangle for the yellow circle.

After the experimenter himself had arranged them in two configurations, an overlapping row of red, green, blue, and yellow circles and an overlapping row made up of a yellow triangle, blue circle, and red square, and asked the patient which arrangement is "prettier," the patient said, "I think that one" (pointing to the circles). Experimenter: "Why?" Patient (after long consideration): "Because they are all this way" (making circular motions with his finger).

From Experiment 2 we see how careful one must be in judging overt results. Although at the beginning it seemed certain that the patient had finally "arrived at the form," this

supposition was proved false by his reference to the difference in color of the circles. He obviously wanted to proceed "quite differently" from the way he did in the preceding experiment; but he remained nevertheless oriented according to "color." Whereas at first he had been determined by the *similarity* of color, now he was reacting ("quite differently") to the *difference* of color. The similarity of form in the circles played a decidedly secondary role. This is proved by the fact that the yellow circle could be replaced by a yellow triangle. Not until the two rows were placed beside each other was the patient thereby enabled to make the "transition" to the element of form. And even this transition does not take place in the "conceptual sphere," but remains palpable—"because they are all this way" (makes circular motions). It is extremely characteristic for the behavior of the patient that this "transition" is so difficult for him. The circumstances must be absolutely "coercive" in order to bring about the shift: "color" → "form." This is in pronounced contrast to the reaction of normal persons, in whom, as we already mentioned, the shift followed immediately without any change of the original conditions.

Experiment 3. In order to emphasize still more the immediate way in which likeness of form suddenly thrusts itself phenomenally upon the apprehension of the subject, we arranged the following experiment. The patient was given all 12 figures face downward. The backs were all white. He was asked to sort them. The patient observed first: "All white." Then, fairly quickly, he put together all those of like form in three separate rows. The experiment confirmed our expectations completely. *Now that a phenomenal organization on the basis of color is no longer possible, the "element of form" immediately obtrudes;* i.e., "the sphere of color," which does not permit homogeneous organizations, recedes and a new sphere, highly organized within itself, namely that of forms, becomes the prevailing stimulus.

Parallel experiments on normal persons. This type of reaction is also found in normal persons. As soon as the figures are presented to the subjects, they exclaim: "Oh, everything is white," or "Now everything is dead," etc. Then they immediately add: "Now there is no choice," "The identical figures thrust themselves upon one right away." Thus it also holds true for normal persons that this stimulative constellation is palpably coercive in the same way as it is for patients.

The three figures with which we had worked in our experiments up to this point were all *identical (congruent) in form.*

Revesz (5) in certain experiments in abstraction on monkeys, succeeded in training his two experimental animals so that they reacted in the desired manner to figures which were identical in form (different in color). However, as soon as Revesz gave them only similar instead of identical figures (for example, triangle), the monkeys failed. In order to test the behavior of our patient under this change of experimental conditions, we tried the following experiment.

Experiment 4. The patient is given 8 figures of different color and form: *three triangles* (one red acute-angled, one yellow equilateral, one green obtuse angled), *three quadrangles* (one red oblique-angled parallelogram, one yellow oblong, one green square), *two circles* (red and yellow). *Although Experiment 3 (on the organization of forms) had preceded this one, the patient immediately put the figures of like color together.*

Experiment 5. When, however, all the figures were shown with the white side up, and no specific directions given, the patient arranged them in a line from left to right with the largest figure on the extreme left and gradually decreasing in size. The figures lay in the following order from left to right: oblong, oblique, square, circle, equilateral, acute, circle, and obtuse. When asked what this method of arrangement meant, he made an angle with his hands, signifying *that he had put the figures together according to size, "in the form of a wedge."*

Parallel experiments on normal persons. The results of experiments on normal persons also offer proof for the immediacy of the method of arrangement according to size. One normal subject, when told to sort the figures (white side up) with as little reflection as possible, carried out the task exactly as the patient had done. His behavior too was determined by the size or the wedge-like form, although he had not formed any clear concept beforehand of what he was doing. Then, however—when the experimenter said one could also arrange the figures differently—the subject *immediately* put them together *according to form.* Another subject expressed himself about the experimental arrangement of the figures white side up thus: "This is a very unpleasant situation, actually painful. Really the main impression is that of white. One can put the triangles together too. They have something in common. The two circles are pleasant also. The parallelogram doesn't fit with the two quadrangles." We see that in this case the experience of the "forms" in palpable material is not at all "coercive" even for normal persons; on the con-

trary, it is unpleasant. It is necessary to overcome this first impression in order to arrive at an orientation toward the formal element. But, whereas in the normal person this shift is easily accomplished, in the patient it is extremely difficult.

Experiment 6. Since in Experiment 5 all the insistent urging to sort "differently" did not help the patient at all, the experimenter took these figures away and put back the original arrangement (see Experiment 3) of figures with identical forms, white side up. *Without hesitation the patient put the figures of like form together.* Then these figures were taken away and the patient was again given those of Experiment 5 (white side up). He was told to sort them "just as" he did those of a moment ago. The patient hesitated a long time; he constantly tended to arrange them once more according to size, *until he suddenly exclaimed "Ah!" and now he immediately put together the three triangles, two circles, square and oblong.* He held the parallelogram in his hand for some time, then put it with the oblong, with the explanation: "a little crooked" (meaning, just like the oblong, only crooked).

If we compare the result of Experiments 4–6 with the preceding, we can establish the fact that the patient in both cases reacted identically. In these last experiments too he was determined first by the "element of color" (in spite of the preceding experiments in form). But it was especially difficult this time to bring about a reaction to the formal element. An appropriate experiment with especially coercive conditions was necessary. From the explanation with which he put the parallelogram with the two other quadrangles (it was for him "just like the oblong, only somewhat crooked"), we can conclude that here again he did not proceed with the help of a principle of classification, but that he put the figures together only according to their palpable relatedness with regard to form. We found this same hesitancy in the normal person in placing the parallelogram only when he was acting with as little reflection as possible. Yet as soon as the figures were sorted according to the general principle, "form," and to the more specialized principle, "quadrangularity," the parallelogram offered no greater difficulties than the square or the oblong. This kind of reaction, however, was lacking in the patient, who constantly tried to achieve a kind of arrangement which would be natural for him, namely the concretely phenomenal arrangement according to size. As already mentioned, there was no question in this experiment of a *voluntary* transition from color to form in the patient; but

rather, the way in which the figures acquired pertinence was *dictated* to him by what was palpably manifest.

Summary of the color-form-sorting experiments. In the introduction we have already formulated the question as to the processes on the basis of which "sortings according to various points of view" are obtained in patients or normal persons. In connection with the experimental results of this paragraph we are now able to state the following. Under our particular experimental conditions the normal persons (in so far as they proceeded conceptually) easily succeeded in sorting the given cardboard figures both according to color and according to form. The transition or, as one might say, the "point of shift" from one method of sorting to the other offered no particular difficulty.

Although in our patient some kind of arrangement of the figures could always be achieved, the difference between his reaction and that of the normal person consisted in the fact that he could not volitionally give up the kind of arrangement which he had just achieved in favor of another. As we have seen, it was not easy for him to pass from putting together figures of like color to putting together figures of like form. To obtain from the patient as prompt a transition from one way of reacting to another as from the normal person, the purely verbal direction "to sort differently" was not sufficient. In general it was necessary to use particularly coercive factors, rooted in palpable immediacy. Thus, in the patient the impulse to change his method of procedure never started from conceptual considerations, but always lay in factors of a purely palpable kind.

There were *three kinds* of condition which brought the patient from his orientation toward organizations within one particular "sphere" to organizations within another "sphere" intended by the experimenter. First, the patient was brought to a "point of shift" by the *juxtaposition* of figures of different color and like form, or of figures of different color and different form (see Experiment 2). Second, the transition from "size" to "form" was achieved by the *after-effect* of a corresponding preliminary experiment (see Experiment 6). Third, the *"homogeneity,"* the *"impossibility of organization"* within an experienced area served as a condition for the appearance of a "point of shift" (see Experiment 3).

The behavior of the patient in the experiment with the cardboard figures is no more to be considered as a sorting in the ordinary sense of the word than his behavior with the skeins of wool. It is rather a matter of direct reaction to organizations of a palpable kind, the comprehension of which does not presuppose a "categorial" attitude. The correctness of our interpretation is confirmed especially by the parallel experiments on normal persons. From these it is evident that normal persons behave quite similarly to patients when they act quickly with as little reflection as possible. In such behavior it is always evident that the change in the type of reaction is the expression of a "point of shift" in the phenomenal realm. The change does not occur through a volitional shifting on the part of the subject.

Herewith the phenomenon of the "point of shift" becomes the focus of our consideration. The way in which the point of shift appears upon the scene and the conditions of its appearance must give us a hint about the way in which those concrete performances are brought about, hitherto usually considered to be the product of particular processes of abstraction.

The following investigations concerning the sorting of objects seem especially suited to add to the further clarification of this problem.

SORTING OF OBJECTS

The experimental set-up described and illustrated below differs from those previously mentioned in that it was always used routinely as a standard set-up in the investigation of a great number of subjects. We had to keep the external conditions of our experiment constant throughout in order to be able to compare and correlate the reactions of the various subjects.[4]

The objects were chosen and arranged according to quite definite points of view, which were calculated to facilitate closer investigation of the problem of the point of shift. We were chiefly concerned with choosing numerous experimental objects of such a kind that in certain respects they could be "experienced as fitting together very well," and in others "as fitting together very poorly."

[4] With the help of a checked table cloth it was possible for us to arrange the experimental objects in the same way in every experiment.

METHOD AND EXPERIMENTAL ARRANGEMENT OF OBJECTS

In the system of the 30 experimental objects employed we included various kinds of possible organizations.[5] The objects could be sorted according to their *use:* (1) toys, (2) tools, (3) smoking paraphernalia, (4) edibles, (5) eating utensils; or according to their *color* (one object of each of the above 5 groups was bright red); or according to their *elongated form* (in each of the 5 groups there was one longish object); or according to their *double occurrence* (in each of the 5 groups one object appeared twice, for example two lumps of sugar, etc.). All of these possibilities of sorting were emphasized by the spatial arrangement of objects on the table.

This emphasis on the possible pertinence of the objects to different groups was, however, weakened by the fact that the objects within the several groups stood in a very "paradoxical" relationship to each other. (For example, the doll's spoon did not go with the ordinary knife and fork, the child's hammer was much too small for the large nail, etc.)

We can, of course, cite only the main points of view which guided us in the construction of our experimental set-up. Everything else is taken up in the discussion of the protocols.

Instructions. The subjects were first asked to look over the set-up and to make themselves familiar with the individual objects. They were permitted to ask about unknown objects. Then they were instructed to choose any object at all. Children were told to choose "the prettiest," "the one they liked best," and to hand it to the experimenter. From now on we shall designate this object as the "first object of departure." After this was done, the subjects were to hand to the experimenter those objects which, in their opinion, "belonged with" the chosen object of departure, or "went with it," or "which one could suitably hand over with it." These expressions were used together in this order, especially at the beginning. Each time an object was given to the experimenter it was removed from the sight of the subject. When the subjects thought that "nothing more belonged with, suited, etc., the object of departure," the "series" was considered closed, and all the objects were returned to their original positions. Next a new object of departure was chosen, not by the subject, but by the experimenter. Again the subjects were to hand the experimenter the proper objects. The forming of "series" was continued as long as the experimenter deemed it necessary. This phase of the experiment we shall designate in the following as the phase of "handing."

At the close of this part of the experiment the original arrangement of objects was reconstructed. The subject was then told to sort all the objects in clearly separated groups in the way in which the objects seemed to him to "belong" or "fit together." For this purpose the subject was given a particular place beside the table cloth (on which the objects were set up), where he was to distribute the objects in the aforesaid manner. When this was accomplished, the "phase of sorting" was finished. This whole experiment we shall designate as the main experiment. After its completion supplementary experiments of various kinds were made, according to whether particular questions presented themselves with regard to the individual subject.

[5] List of experimental objects: toy wooden sword, toy dachshund, pipe, match box with red cover, Virginia cigar, two broken-off matches, chocolate cigar, two forks, knife, red and yellow cardboard discs, red wooden plate, ball, red apple, slice of bread, two lumps of sugar, doll's spoon, screw driver with red handle, child's hammer, pair of pincers, padlock, bicycle bell, red and white candles, piece of wood with nail in it.

Subjects. Forty kindergarten children (4–6 years old), 30 feeble-minded children of a Frankfurt ungraded school (ca. 3–13), 6 patients with cerebral lesions, and 12 normal adults, most of whom were psychologically trained.

Behavior of Patient R. After the patient, *without saying a word*, had followed the instruction to look at all the objects (he was expressly told he need not name them), he was asked to hand to the experimenter any one of the objects. The patient chose a lump of sugar. Now he was directed to hand over all of the experimental objects which seemed to fit with the object of departure. After minutely examining the array, the patient shook his head and said "nothing" belongs with it. Thereupon the experimenter picked up, one at a time, 20 different objects from the table and each time instructed the patient to hand over whatever objects fit with it. In 12 cases the patient denied emphatically that anything went with the object in question. In 7 cases he handed over one object for each object of departure (for example, the fork for the knife, the match box for the cigar, the pincers for the piece of wood with a nail in it). Once he handed over 2 objects (for the fork, the doll's spoon and the knife). This concluded the phase of "handing."

When we consider the pairs of objects which the patient formed several times in this procedure, the type of combination (for example, knife-bread, etc.) seems extremely concrete. The patient usually put the objects together *in a way in which they could actually be used together in the situation of the moment.* Since, by our choice of objects, we had managed that combinations of this kind should be possible only among two or at most three objects, and often not at all, it is not surprising that the patient could not arrive at any real sorting so long as he retained this concrete attitude. Since he was oriented solely toward the manipulative element in the objects, they had in general "nothing to do with each other" for him.

Among the other patients with cerebral lesions whom we investigated, the reactions of patient B. (2) particularly resembled those of patient R. He too "handed over" in general not more than three objects for any one object of departure. The practical utility of the objects was for him too the decisive factor. This led to the result that the patient frequently missed some objects in the array presented to him. For example, with the pipe he handed over the matches and the spoon (for scratching out the pipe) with the words, "There is no tobacco." Further, he felt the lack of the key for the padlock, etc. The patient showed this kind of behavior during the whole first phase of the experiment. When, however, the experimenter asked him more and more urgently whether one must always hand over in that way, whether one couldn't do it *differently*, and "what things, for example, one might hand with this [the fork], *which are like it*," then the patient suddenly said, "Oh!" and gave him the second fork. As this still did not seem to satisfy the experimenter, the patient suddenly nodded understandingly at the next object of departure and gave him, one after another, all the objects which were made partly or entirely of metal. From then on this method of sorting determined most of his reactions in handing. In "sorting" also, he put together in groups the objects made of

metal, paper, wood, and the edibles. We shall return later to this method of sorting. It seems to be particularly natural for this patient, since in the Gelb-Goldstein experiments he favored this type of reaction as well.

The reactions of patient R. in the next phase of the experiment, that of sorting, differ scarcely at all from those just described. When instructed to sort the objects the way they belong together, the patient always picked up from the array one object with both hands, and formed in this way 10 pairs (for example: cigar-matchbox, apple-knife, fork-spoon, etc.). The remaining objects he put down separately (for example: bread, pipe, toy dog, etc.). His reaction time was somewhat longer than that of most of our normal adult subjects.

As we have seen, the patient, in sorting, behaves quite similarly to the way he did in "handing." *He persists in his orientation toward the manipulative element.* It is important for our problem that the patient never spontaneously showed even a tendency to actualize other ways of reacting. When asked whether one could put the things together "differently," he answered in the negative, even after long consideration.

Among the normal adult subjects we find certain parallels for this concrete kind of behavior.[6]

Some interpretive observations one of our normal subjects made may contribute especially to our problem of the "point of shift." As soon as the orientation toward the element of manipulation disappears, this subject experiences the material in a different way. *No longer do objects of common use,* which have more or less to do with each other, lie before him, but now only *"spots of color"* appear to him. We have here, quite unintended by the subject, a spontaneous transition from the "thing sphere" to the "color sphere." It is not the subject who separates, or "abstracts" the colors from the common objects, but the colors suddenly thrust themselves upon him. And this phenomenon evidently occurs after a palpably meaningful organization within the context of "common-use objects" was not able to enforce itself. But this phenomenological experience of the color spots also suddenly yields to another—"the whole falls apart"; a new organization of another sphere enforces itself: "for grown-ups and for children." We shall not discuss here to what extent this

[6] Editors' note: protocol omitted for lack of space.

last distinction is to be considered as more "concrete" or more "categorial." For our purposes it is important that this normal subject, in contrast to the patient R., is able to experience quite a number of transitions between spheres of very different kind.

A comparison of the behavior of this subject and the patient R. brings out especially clearly the *rigidity and monotony* in the patient's behavior as contrasted with the flexibility which is always possible in the normal subject.

This difference can be recognized even more pronouncedly in the reactions of the other *normal adult* subjects. In general, they accomplished both the handing and the sorting on the basis of quite definite principles of classification, i.e., on the basis of categorial behavior. Whereas, in sorting, usually only the point of view of pertinence with regard to use was decisive; in "handing," however, the most varied points of view dominated, one after another in quickly shifting succession (for example, "similarity," "color," etc.). "Points of shift" from one sphere to another appeared, so to speak, constantly. Undoubtedly the external impulse for these changes came in general from the purely phenomenal realm, but the necessary condition for the actualization of such points of shift must usually be sought in the ability of the subject to orient himself to the given material as little concretely and as little practically as possible. In contrast to the results obtained from patient R., we could ascertain in most of the normal subjects a far-reaching *independence of the "real" nature* of the articles presented to them. Frequently their reactions could scarcely have turned out differently *if they had had to deal solely with the names of the experimental objects.* This conceptual method, when applied to our set-up of experimental material, necessarily led to the most varied kinds of arrangement. Thus it actually happened that in the beginning most of these subjects had great difficulties each time with the instruction to hand over or sort what belongs with this or that, or what belongs or fits together. The subjects could scarcely ward off the multiplicity of possibilities of arrangement. Only with difficulty were they able to come to a final decision. When the patient R. likewise hesitated to obey the instruction "to hand over" (or "sort"), it was for just the opposite reason; for him the objects usually did not fit together at all or only very poorly.

Behavior of children. We have, up to this point, become acquainted in the main with two types of reaction: (1) *"sorting in pairs"* (the same in handing) or "segregation" of the objects on the basis of *very concrete* orientation toward *manipulation;* (2) *"sorting in large groups"* (and "handing of long series of objects") on the basis of a more *categorial,* conceptual orientation. Now we have also met with types of reaction which *externally* were identical with this second type in that large groups and long series were formed. But they came about on the basis of a kind of orientation which is entirely different from the categorial.

To illustrate this last type of reaction we shall cite some extracts from the protocols of kindergarten and ungraded classes.

Four-year-old Mo. tells us, while handing over the bicycle bell, little red candle, etc., to go with the padlock (as object of departure), "You can make a light in the evening when you put on the lock down in the cellar." And when the experimenter asks her what the bell has to do with this, she answers reproachfully, "Why that's upstairs in the apartment!" Similarly, seven-year-old Ste. accompanies his handing of objects to go with the hammer (as object of departure) with the following words: (He hands the sword.) "You can make that with the little hammer." (He hands the white candle.) "You make a light with that when you hammer." (Hands the match box.) "You light the candle with that." (Hands bread.) "You eat that when you're hungry." (Hands pipe.) "And then you smoke the pipe." (Hands dachshund.) "And the doggy must keep watch when the man goes out."

The ungraded school children under 10 react in general in a similar way. For example Hö. (8 years, 3 months) hands over a number of objects to go with the dachshund (as object of departure) and spontaneously gives the following explanations: (Hands plate.) "He drinks out of that." (Hands little red plate.) "He can sit on that." (Hands ball.) "He plays with that." (Hands bread, 2 lumps of sugar, apple.) "He eats these." (Hands spoon.) "To feed him when he doesn't eat anything." (Hands match box.) "When you want to cook something for the dog."

These few examples are probably sufficient to show that the children frequently handed over most or all of the objects to go with one single object of departure. For the children, "sorting" in the adult sense came into question only in rather rare cases. They understood the instruction in their childlike way. In general, *to belong together* meant to the children "meaningful appurtenance to a situation" and indeed to a situation which from the adult point of view must seem "unreal," "imaginative," "playful." As soon as the attention of the child is attracted by the dachshund, for example, a "story" begins to spin itself about the dog. The main theme is determined by the little dog and the developments are determined by the objects perceived thereafter. It would, of course, be entirely false to maintain that the children "knew," to be sure, that the dachshund was only a "carved bone knickknack," and that they "transformed" it by their fantasy into a "real" animal. This kind of interpretation makes the mistake of identifying the child's world with ours—a mistake which is generally recognized in the child psychology of today. If one observes chil-

dren in such situations (which from our point of view are "imaginative"), one can no longer harbor any doubts about the vividness, the concreteness of the reaction. The "stories" are not invented, thought up, and then superimposed upon the real world, but rather, *they take place in the real world*—only it is in the world of the child, and not in ours.

We can then assert that on the basis of the childlike orientation the number of objects which belonged together could be increased *ad lib.* The objects, which were perceived as already definitely related to some "story" or other, shifted their functions accordingly. The procedure of the children must be designated as *concrete to the highest degree,* and concrete with reference to the experienced reality of the child. Frequently we were able to observe that with the perception of some previously unnoticed object, the manner of sorting or grouping objects suddenly changed the "story" had evidently taken a new turn through the dominance of a new article.

In one normal, psychologically trained adult we found, under certain conditions, types of reaction which showed a certain relationship to those of the children. The statements of this subject will give us certain hints about the experiences of the children.

Subject He.: The subject repeatedly "handed" series of as many as 17 objects and sorted "groups" of as many as 8 objects.

As the first object of departure the subject chose the apple and handed over 16 objects to go with it, in the following order: knife, 2 lumps of sugar, bread, chocolate cigar, Virginia cigar, 2 forks, doll's spoon, pipe, match box, 2 matches, 2 candles, plate. On completing the series she gave us these bits of observation upon herself: "I constantly had a story in mind. By the apple I was led to the situation of eating. This demanded knife, fork, spoon—really a whole cover. In looking for this, I saw these 'impossible' eating utensils and got a real shock. Then I consciously adjusted: 'But they aren't toys'; at this moment the objects were no longer impossible; they fitted the situation of eating; it was as if they were '*changed*.' I went on from one thing to another and could no longer separate. It could have gone on this way for hours—so to speak, a chain of little circles, which hung together."

In another series with the bicycle bell (as object of departure), the subject handed padlock, pincers, hammer, two pegs wood, screw driver, and stated that in handing the lock and pincers she had the idea of a bicycle kit in which these two things belonged. Suddenly—on perceiving the hammer—she was led to the situation of fussing about with gadgets, and this notion caused her to hand two pegs, the wooden board and the screw driver. It struck her especially that the objects had frequently changed their significance according to the particular object of departure to which they were related. Thus, for example, the plate had functioned as "eating plate" (objects of departure: apple, dachshund, and fork), as "toy plate" (objects of departure: little plate and ball), and as "ash tray" (object of departure: Virginia cigar). In "sorting"

she made "drawers" just as if she had had a "cabinet at home with many things to arrange in it." In doing this she formed the following groups: (1) tools, (2) edibles, (3) toys (plate, ball, chocolate cigar, 2 little plates, spoon, sword), (4) table silver, (5) match box, 2 matches, 2 candles, (6) Virginia cigar and pipe.

We have to do here with a kind of behavior which is not entirely like any adult behavior yet described. The subject neither lets herself be disturbed by the fact that objects fit together poorly (like patient R), nor does she feel the objects as very fitting from "conceptual" points of view. The factor which is decisive for her reactions is—as she herself says—the "situation" in which she finds herself at the moment (usually purely imaginative). Thus, for example, in handing objects to go with the Virginia cigar (as object of departure), a perfectly definite smoking table is present *to her imagination,* on which, so to speak, she puts the object which she hands over. Or another time, with the fork as object of departure, she imagines herself setting the table for dinner. Sometimes a sudden transition from one situation to another occurs (see protocol: bicycle kit—fussing about with gadgets). This transition may be caused by the perception of one or of several objects. But there are always "stories" present which successively replace one another. Sometimes, however, it also happens that the subject is completely torn away from a situation by the perception of a particular object. She experiences a "shock." Then suddenly she realizes the real nature of the various objects; the incongruity of certain objects offends her. A conscious shift is necessary in order to get back to the previous situation. If this transfer is attained, the objects seem as if they were "changed"; the disharmony no longer exists.

Such reactions to situations differ from the concrete types of behavior (noted particularly in patient R.), especially in the far-reaching independence of the real nature of the objects. They differ from the categorial behavior, demonstrated by other normal subjects, in the extraordinary vividness of the procedure, and in their entirely irrational nature. Thus this type of reaction is, on the one hand, *concrete to the highest degree* and, on the other hand, *"highly imaginative."* We see here the close relationship of this type of reaction to that which we described in children; and at the same time we gain hereby further understanding of the behavior and the experiences of the little ones. One must

not, however, overlook the fact that this comparison can be only relative. This is particularly true of the "experiences of shock," which are expressions of the fact that the world of imagination must remain to a very large extent "unreal" for us grown-ups. In a little child a transition of such a rude nature is impossible, because his "actual" reality is by no means so sharply distinguished from the "reality of imagination."

We scarcely need mention that the types of behavior here described do not nearly exhaust the material which actually occurs and which we observed. We could set up a whole series of steps of different "reaction tendencies," rising from the concrete to the more categorial. The selection given from our material, however, will suffice to form a basis for the discussion of our specific problem.

Summary of the experiments in the sorting of objects. In looking back over the experiments in sorting which were performed upon the patient, let us ask ourselves what points of view have been furnished for the investigation of our problem of the "point of shift." In the main, the answer must be a negative one. Under our experimental conditions, the point of shift occurred not at all or very seldom. This fact is due to the circumstance that in offering the patient (especially patient R.) objects of common use, we forced him from the very beginning into an orientation toward a perfectly definite sphere. The patient concentrates entirely on the practical use of the articles. He tends to "do something with them," to "manipulate" them. Thus it follows that for the patient the objects will fit together not on the basis of any common elements, but on the basis of the way they "can be used together" practically (as palpably experienced). Considering the special nature of our experimental conditions, very narrow limits are set for any change in the patient's type of reaction. And this was actually to be seen in a kind of "monotony" or "fixedness" of his behavior.

On the other hand, we found a significantly greater flexibility in the normal adult subjects. Even when they preferred certain definite types of arrangement if they acted unreflectingly, nevertheless a reorientation toward spheres of the most varied kinds was easily possible for them if the experimenter asked whether one could not also sort the things "differently." Some subjects

even changed their types of sorting solely on the basis of conceptual knowledge, by asking themselves what possibilities of arrangement—purely formally—existed. Here we have to do with "points of shift" of a purely "categorial" nature. We also observed in the normal adult subjects (especially when they were acting unreflectingly) transitions from one sphere to another, even within the purely phenomenological realm. And finally a third kind of "point of shift" should be mentioned, which was caused by definite palpable experiences and which then led to a principle of classification, formulated conceptually (as, for example, when the experience of a red spot of color gave rise to "sorting according to color" in the categorial sense).

SORTING OF OBJECTS UNDER PARTICULARLY "COERCIVE" CONDITIONS

Up to this point our experiments on patient R., in the sorting of objects, have led only to negative results concerning the "point of shift." We should like now to investigate whether there are conditions—and if so of what kind they may be—under which the patient can be led to shift to other types of behavior with regard to objects of common use.

Experiments in threes. Just as we previously presented the patient with three colored skeins, we now present him with three objects, for example: Virginia cigar—match box—pipe. The patient is to put together those two objects which "fit together best." He decides, in this case, on the Virginia cigar and match box. He refuses to put the pipe with the match box, offering as explanation: "because there is no tobacco in it." The following statements of the patient show how strongly he is still oriented in these experiments toward practical usableness, realizable in the situation of the moment: the knife belongs with the apple rather than with the fork; the bread goes with the knife, but not with the sugar (whereas numerous other subjects felt immediately that bread and sugar belonged together as "edibles"). In contrast to most of the other subjects, he does not even consider putting the chocolate cigar and the match box together. Of the two possible combinations, only the one "close to life" is coercive for him. Thus, this sort of experimental condition brought us no further with the patient with regard to the "point of shift."

Sorting of objects according to "material." The patient is presented with two piles of objects. In the one are only objects made of wood (stethoscope, two pen holders, handle of a hammer, pencil, two spatulas); in the other pile are only objects made of metal (padlock, doll's spoon, bicycle bell, razor, two nails, pair of pincers). The patient is to say whether he finds "any sense in this arrangement," whether it "is right this way."

For a long time he looks at the set-up completely puzzled. Even the assurance of the experimenter that there are quite definite common elements among the objects in each pile does not add anything to the comprehension of the patient.

Experiment 1. The experimenter takes a nail out of the one pile and the handle of a hammer out of the other, and asks the patient to put these objects where they belong among the other objects. He puts the nail with the wood and the handle with the metal. When asked why he did it this way, he says, "Well, Doctor, you, that way too." (He means to say that the experimenter took the objects from these positions.) The experimenter repeats the experiment with the same objects, but in such a way that the patient has no idea where they were taken from. Uncertain and hesitating, the patient glances back and forth from the objects in his hands to the two piles. Finally he decides (still very uncertain), and puts the nail with the wood. Immediately thereupon he evidently intends to put the hammer handle with the metal, but suddenly stops, and cries out animatedly, "Oh, now I know." Absolutely certain and clearly conscious of his purpose, he now puts the two objects on their proper piles and says, "Wood—iron."

This experiment teaches us that there is another kind of reaction possible for the patient, besides that already mentioned, but that it is realizable only under quite definite, very "coercive" conditions. The entire lack of voluntary action here is very interesting. *The patient does not come by himself to the point of: "wood—iron," but this pair forces itself upon him phenomenally;* he is completely surprised by it. The abruptness expresses itself unmistakably in the whole behavior of the patient. His features suddenly change; he strikes his head with his palm; he has an absolute need to explain his experience to the experimenter (which is made difficult by his aphasia).

Experiment 2. In order to ascertain whether this comprehension of the connection "wood—iron" actually made it possible for the patient to sort according to "wood" and "iron," we mix up the objects of the two piles, present them unarranged to the patient, and instruct him to sort them. Without hesitation he obeys the instruction and puts the two piles together again quite correctly. In doing so he proceeds very carefully and pedantically, examining every object very exactly to see of what material it is made.

On first glance the result of this experiment does not seem to agree with our assertion that it is impossible for the patient to sort objects according to a principle of classification. *It looks as though he had actually sorted according to "material" like a normal person.* If the patient had really succeeded in doing so, if he had finally arrived at the principle, then it would make no

difference to him if he were given other materials to sort, besides wood and iron.

Experiment 3. The following objects are added to those already used: a rubber ball, a blotter, a clean piece of paper, a full ink well. All the objects are mixed up and presented to the patient unarranged; the instructions are the same as in Experiment 2. The patient looks at the set-up for a long time and then hesitatingly takes the ink well and puts it in the left hand corner of the table; then he puts the other objects in a horizontal row along the edge of the table at intervals of several centimeters (ink well—stethoscope, standing up—two pencils, together—rubber ball—handle of hammer—two pen holders—bicycle bell—pincers—padlock—paper).

We were extremely surprised by this kind of arrangement. Our efforts to obtain from the patient a clear explanation of his motives for it were unsuccessful. Evidently it was for him only a question of "putting the things in order." The sequence of the objects, as such, had no special significance. This impression was confirmed when the experimenter changed the order of some of the objects and asked the patient "whether one could do it that way too." The patient immediately agreed to this exchange of position.

Since the patient had seemingly forgotten his reaction of Experiment 2, the experimenter mixed up these same objects, presented them to him again, and instructed him to sort them just as he had done before (in Experiment 2). These hints, however, were unsuccessful. The patient again took the ink well and started a new series with it, similar to the one in Experiment 3. He did not get the idea of sorting according to "material."

Experiment 4. After the four new objects are removed, the patient looks over the set-up for a short time, then suddenly says, "Oh," with an understanding smile, and promptly arranges two piles: wood—iron. The experimenter now takes the nail out of the iron pile and puts it with the wood. Then he asks the patient whether one could do it that way too. The patient shakes his head and puts the nail back with the iron.

The result of this experiment teaches us that the changed reaction in Experiment 3 cannot simply be attributed to the patient's *"forgetting"* a principle of classification (material), which he had once grasped. *Rather, the addition of the 4 objects had evidently modified the set-up for the patient in such a way that the distinction: wood—iron, was phenomenologically no longer present for him.*

Experiment 5. The circumstance that the patient in Experiment 3 continually stretched out his hand toward the ink well, led us to suspect that his changed behavior was caused by the large size and conspicuousness of this object. The experimenter, therefore, mixes up the objects of Experiment 4, adds only the ink well, and presents them to the patient. Looking over the set-up, the patient suddenly sees the ink well and reacts just as in Experiment 3. He starts a series of objects beginning with the ink well. Nor do the repeated instructions of the experimenter to sort just as before (Experiment 4) distract him from this type of reaction.

The experimenter again presents the original set-up (Experiment 5) and expressly instructs the patient to pay no attention to the ink well. But the patient does not succeed in so doing until the experimenter covers it with his hand. *Now the patient again arranges two piles.* After he is finished, the experimenter uncovers the ink well and points to it with a questioning gesture. At first the patient is stuck; then, however, he says quite suddenly, *"Oh, glass," and places the ink well by itself.*

Having finally got the patient, on the basis of appropriate changes of the objective experimental conditions to put together objects of common use which were made of the same material, we might now expect that the patient, under analogously "coercive conditions," would also put together objects of the same "color."

Sorting of objects according to "color." Actually we did succeed in effecting this reaction in the patient in the following way. We gave the patient two piles of objects of common use. The one pile consisted of red objects, the other of white. The patient was to state whether this kind of arrangement made any sense to him. At first he answered in the negative (even after we had had him name the color of each separate object). Not until, as in the wood-iron experiment, he had to decide where to put a red object and a white object, did he suddenly cry out, *"Oh, because it's white," and put the two objects on the corresponding piles.*

These experiments too we performed on normal persons, and were able to establish that there are sometimes even normal people for whom this type of "sorting" does not immediately appear obvious. With these persons, however, the mere mention of the word "color" is sufficient to cause the "point of shift" to occur immediately.

The patient's great difficulty in shifting easily manifested itself also in the test with the well-known Rubin figures (6). A quick shift (of the vase and of the faces or of the black and white crosses) was quite impossible for the patient. Here too the

change of figure and background was achieved only under especially "coercive" experimental conditions, and only after a disproportionately long reaction time.

Summary of the experiments in the sorting of objects under particularly "coercive" conditions. From the point of view of external results, we had got patient R—by the special kind of experimental conditions used—to "sort" according to "material" and "color," disregarding other connections of the objects. The closer analysis of the experiments, however, showed that it was impossible for the patient to classify the single objects with one or the other pile as representatives of corresponding categories ("material" or "color"). We were, on the contrary, able to establish that the same concrete, palpable factors which had at first influenced him toward "manipulative" groupings, later—under changed conditions—caused him to form "a wooden and an iron" or "a red and a white" pile. In both cases the mere juxtaposition of wood—iron (or red—white) was not sufficient to make the patient shift from the sphere of "things" which one "manipulates" to the sphere of material or color. Whereas the majority of the normal adults whom we investigated felt such juxtaposition to be especially "obtrusive" and "obvious," the patient, on the other hand, needed a more forceful foundation. With him the "point of shift" did not appear until he was about to put a single wooden object with the metal pile (or a red with a white).

Only under these conditions did a new *phenomenal* kind of organization force itself upon the patient. The experiment in which the patient suddenly, so to speak, "discovered" a third category of material (glass), is likewise typical of a change within the purely palpable realm. After the wooden and iron piles were finally put together by the patient, the ink well, when suddenly added, lost its character as a utilitarian object—it became "glass."

One might ask at this point whether and in what way the "points of shift" are experienced by the patient. Since one can scarcely get any statements from the patient himself on this subject, one is forced to depend on observation of his behavior at the moment when his type of reaction changes. One can certainly conclude from the lighting up in his facial expression, frequently observable on such occasions, and from his joyful

exclamations that something happens in the patient. It may, nevertheless, be something quite different from that which happens in the normal person in the same situation. A clue in this direction is perhaps to be found in the fact that the patient, after the point of shift, usually remembers nothing of his previous reaction; even reminders on the part of the experimenter do not help. *For the patient, the organizations within one sphere evidently have nothing to do with those of the other spheres.* His joy on the occasion of a new reaction does not, then, arise—as in the normal person—from the experience of having "grasped" a new connection; it seems rather to be the expression of the experience of a new situation closer to the performance demanded of him.

SUMMARY OF THE RESULTS CONCERNING SORTING

We found in our various experiments that the results of the Gelb-Goldstein work, described in the introduction, were confirmed. Under our experimental conditions, the reactions of the patients whom we investigated (especially patient R.), were determined only by organizations and groupings which forced themselves upon the patients phenomenally. This fact was shown (a) in the experiment with the Holmgren wool samples, in which only the identical skeins "fitted together", and (b) in the different experiments with the cardboard figures; in the latter the instruction "to sort" was first understood by the patient in the sense of "to build, to make patterns," and only through appropriate "coercive" conditions did the quality of "color" or of "form" or of "size" become effective. Finally it was also shown (c) in the experiments with objects, which at first produced in the patient only one and the same reaction (namely, the possibility of using the objects together), and in which again only under especially "coercive" conditions a grouping of objects of like material and like color was attained.

Again and again we were able to convince ourselves of the lack of real principles of sorting in the patient—first by numerous control experiments, and second by comparison with the reactions of normal adult subjects. Although for the normal adult subjects too there were palpable kinds of organization which were so "pregnant", so coercive, that the subjects, when left to them-

selves, reacted to them with only one type of sorting; nevertheless, even in these cases a slight hint on the part of the experimenter was sufficient to occasion in the subjects other types of reaction. In contrast to this behavior, the patient could make nothing of such aids on the part of the experimenter, since it was impossible for him to change a phenomenal organization, once experienced, i.e., to arrive at a "point of shift" by means of the conceptual realm.

In general, our normal adult subjects behaved "categorially" in sorting objects. With a little effortless consideration they discovered among the experimental objects a more or less large number of common qualities, which then served them as principles of classification. Frequently they proceeded as if only the names of the objects to be sorted were at their disposal; i.e., the experimental objects were for them only representatives of certain categories. We were sometimes even able to observe that the subjects, when asked "how one might sort differently," no longer paid any attention to the set-up, but simply enumerated in a formal, logical way: "according to color, form, etc." As Gelb and Goldstein (2) have already found, normal subjects are relatively independent of the "real" nature of objects. On the basis of conceptual knowledge they can, so to speak, superimpose a type of arrangement upon objects.

This type of behavior of the normal adult subjects varied, however, according to the degree to which they succeeded in attaining a naïve, unreflecting attitude toward the experiment. This circumstance was evidenced both by the result and by the experience of the subjects. Sorting according to definite points of view was neglected in favor of "immediate" reactions to elments of a concrete sort.

The results obtained from the experiments on children cast a new light on the contrast between concrete and categorial behavior. The concrete nature of patient R (under our experimental conditions) expressed itself in great "monotony" of behavior and in a "lack of facility in shifting"; the concrete nature of the children expressed itself quite differently. The single objects were constantly new stimuli for the children in working out and giving detail to the "stories" experienced at the moment. Thereby their reactions took on a constantly changing, lively character.

The conditions necessary for the occurrence of the "points of shift" in patient R. varied, corresponding to the context in which he experienced the phenomenal organization in question. Thus in one of the experiments with the cardboard figures, the *juxtaposition* of two differently organized "constellations" was sufficient to cause a shift from the "sphere of color" to the "sphere of form." In another experiment the point of shift from the sphere of "size" to the sphere of "form" was achieved by the *after-effect* of a previous experiment with especially coercive conditions. As a further very important necessary condition for the shift, we became acquainted with the *lack of possibility of organization within the sphere just experienced.* Our experiments with objects of common use showed that the shift from one palpable organization to another depends to a high degree on the *nature of the experimental material.* The patient needed stronger support than in the experiments just described in order to shift from "co-usability" to "likeness of material" or "likeness of color."

In the reactions of the normal adult subjects we were able to distinguish *three kinds of "points of shift":* first, a change within the purely *phenomenal realm* (analogous to the points of shift in the patient); further, *a shift from the sensory, palpable* realm (for example, "coloredness") to the *conceptual* (for example, "category of color"); finally, *"points of shift" within the categorial realm* (for example, from "color" to "form"). When the shift took place within the phenomenal realm, it was always a matter of *experiences "of immediate and unreflective nature"*—as statements of numerous subjects confirmed—of certain organizations forcing themselves on the subject. The special conditions for the occurrence of these different kinds of points of shift are extremely varied. We must be contented here with the statement that in the main *two kinds of factor* come into question: first *phenomenal* (of a kind similar to those which we discussed with regard to the patient), and further *elements grounded in the conceptual knowledge of the individual.*

With the children the turning points remained on the whole *within concrete spheres.* They were usually caused by the "discovery" of a new, "especially interesting" object, which then of itself gave a new turn to the story already begun.

From our results up to this point certain conclusions concerning the "problem of abstraction" may already be drawn. The usual

theories of abstraction—although they differ from each other more or less in detail—all have this *basic point of view* in common: the reaction to "partial conformities of several objects of perception (so-called common partial contents)" must be preceded by a stage in which these "percepts" are experientially present as definite objects with a number of well-defined given properties. And the actual "feat of abstraction" consists in *either voluntarily or involuntarily singling out, i.e. "abstracting the common partial content," from all the de facto given contents* (so-called "distinctio rationis").

But this sort of interpretation does not correspond at all to the psychological facts which we have ascertained, in so far as *concrete kinds of response* are concerned. With respect to the concrete responses, such a "distinctio rationis" as implied in the theories just mentioned is out of the question. Here neither the behavior nor the statements of our subjects gave evidence for the presence of this sort of abstraction.[7] The concrete behavior, therefore, does not involve "any voluntary abstraction" nor is there any cogent reason for postulating so-called "involuntary processes of abstraction" as having any part. If we keep to the factual findings of our behavioral analysis, we see that the subjects—whenever they behave concretely—responded each time to a definite organization within a given unreflectively experienced sphere. When this sphere changed for the subject, there were also changes both as to the experience of the contents and as to the experience of the congruencies among the contents of the sphere. So long as we are responding concretely to a stimulus configuration within a definite unreflectively experienced sphere, we can react only to organizations of *this sphere* and not to organizations of any other sphere. Thus it is impossible, for example, to experience the congruency between an apple and a wooden plate "as to color likeness" so long as one is experiencing an apple, a wooden plate as "these things" in a *concrete* manner or situation. While behaving concretely we actually experience a living reality, two definite things—an apple, a wooden plate; if, however, we experience the colors on a concrete basis, then the phenomenal sphere has become changed and we

[7] In order to avoid misinterpretation the Editors have in a few instances altered the original wording of this paragraph.

no longer experience an apple, a wooden plate as just "these things," but as a homogeneous color sphere; that is, in concrete behavior we respond either to things or to colors, but we do not respond to two different spheres simultaneously. Each sphere has its special structural (configurational) laws according to which it organizes itself. The concrete experience of these unmediated spheres is of an unreflected nature (phenomenality), thus not derivable from processes of abstraction. *When, however, we speak of having an experience of an object as "experiencing the object with reference to something" then we use a conceptual formulation which in no way can be identified with the actual facts of concrete behavior.* Why we use and how we are able to use such formulation and behave conceptually, is a question which can be clarified only in connection with the problem of abstract or categorical behavior. This problem does not fall within the framework of the present paper.[8]

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[8] The Editors have omitted the last two paragraphs of the original article, because in them Weigl anticipated some experimental questions of a subsequent paper.

POLITICAL FRAMES OF REFERENCE AS A FACTOR INFLUENCING RECOGNITION *

BY ALLEN L. EDWARDS
Northwestern University

The Theoretical Setting

THE soundness of any position regarding the Freudian theory of memory in terms of pleasantness and unpleasantness depends of course upon the correctness of the assumptions which it makes concerning the relationship between pleasantness and unpleasantness and the ego's desires, beliefs, attitudes, etc. Freud (9), while granting that unpleasant experiences which conflict with the ego's desires will tend to be forgotten, also remarks that "forgetting in general . . . is quite a different problem, in which the defence against painful associations plays a certain part, but is far from explaining everything."

The point as we see it is that any experience which conflicts with the ego's desires will tend to be forgotten more readily than those which do not. An experience is excluded from consciousness not because it is unpleasant, but rather because it involves conflict. Conflict may be accompanied or followed by unpleasantness, but it is not the unpleasantness as such which is the determiner of forgetting; it is the conflict. Thus the question of pleasantness or unpleasantness of the experience is another matter and one of secondary importance as far as our interpretation of the Freudian theory is concerned. Our point of emphasis is to determine the presence or absence of conflict.

Sherif (10) has used the concept of *frame of reference* [1] to designate the resultant "interiorization" of social norms, values, beliefs, attitudes, desires, etc., in the individual "as a consequence of his contact with the social world around him." Frames of reference, once established in the individual, "enter as important factors to determine or modify his reactions to situations that he will face later." We propose that the Freudian theory of memory may be readily interpreted in terms of the

* This paper, in part, was presented before the Midwestern Psychological Association, Chicago, Illinois, May 3, 1940.

[1] Frame of reference appears to be a broad concept related to many psychological problems. Cf. Allport (1), Cantril (2), Edwards (3; 4), and Sherif (10).

conflict or harmony of experiences with respect to the frames of reference of the individual involved. More specifically, our hypothesis is that experiences which are in harmony with an existing frame of reference will tend to be learned and remembered better than those which are in conflict with it.

It should not be concluded, however, that all pleasant experiences must be, by definition, in harmony with a frame of reference or that all unpleasant experiences must be in conflict with one. Many experiences though pleasant or unpleasant do not harmonize or conflict with a frame of reference in any appreciable degree (e.g., a cigarette burn, finding a sum of money which has been misplaced, bumping against a door, etc.). This fact may, in part, account for some of the divergent findings of various investigations of pleasantness and unpleasantness as related to the retention of experiences from everyday life. The investigator, instead of taking into consideration the frames of reference of his subjects, has used their ratings of affective tone as the basis of his experimental design. *Some* of the experiences rated as unpleasant by his subjects probably did involve a conflict, whereas perhaps others did not. At the same time *some* of the experiences rated as pleasant were perhaps readily assimilated into a frame of reference, and others may not have involved a frame of reference at all. It is our belief that in such a case, the experiences—regardless of any qualitative distinction in their affective tone—which were in conflict with a frame of reference would tend to be the ones which were forgotten, while the experiences which were readily assimilated into a frame of reference would tend to be the ones which were remembered. Thus in any study of the retention of experiences from everyday life, the mere determination of the affective tone of the experiences is not necessarily sufficient grounds upon which to base predictions concerning what will be remembered and what will be forgotten. Nor can one clearly evaluate the results of such a study in terms of the Freudian theory as we have interpreted it.

An approach to the problem can be found, however, in a recent study by Watson and Hartmann (13). These investigators compared the recall of atheistic and theistic material for subjects with strong atheistic and theistic attitudes. They found

that material which supported a subject's attitude was recalled much more effectively than material which was opposed to his attitude. Their findings, however, while consistent, did not meet the requirements of statistical reliability. It was the purpose of the present study to investigate further the relationships between a specific frame of reference and the recognition of materials specifically related to that frame of reference.

CONDITIONS OF THE INVESTIGATION

The problem. In this experiment we shall attempt to test the following hypothesis concerning an ego-involved frame of reference and the role that it plays in learning and retention:[2]

HYPOTHESIS

Experiences which harmonize with an existing frame of reference will tend to be learned and remembered better than experiences which conflict with the same frame of reference.

It is assumed for the purposes of this investigation that an individual's expression of an attitude on a particular subject is an indication of the nature of his frame of reference with respect to that subject.

The learning materials. It was necessary to construct a verbal passage which would contain statements in conflict with the frames of reference of the individuals being studied. At the same time it was necessary that the passage contain statements which would harmonize with the same frame of reference. Thus the passage should be neither entirely favorable to the topic it discussed nor entirely unfavorable, but should include both favorable and unfavorable statements. With this feature in view a passage concerning the New Deal and communism was composed. The passage, approximately 1920 words long, required about 10 minutes to read aloud.

The recognition test. Fifty true-false items based on the passage were constructed. All of the items referred to some point made in the passage concerning the New Deal. It was necessary, for reasons which will become obvious in the discussion which follows, to determine in advance how individuals who were strongly in favor of the New Deal would answer the

[2] We are here differentiating between ego-involved frames and perceptual-motor frames, i.e., familiar space systems. It is probable that both influence learning and retention, but we are here concerned only with the former. Cf. Sherif (10).

questions. For this purpose 6 members of the faculty at Northwestern University were asked to answer the questions as a person strongly in favor of the New Deal would answer them.

No one of the 6 judges answering the questions was aware of the answers given by any other judge. After all 6 had answered the questions, the number marking each item true or false was found. There was complete accord on 44 of the items, and on 2 others only 1 judge disagreed with the other 5. The 4 items which showed marked disagreement were discarded. There were thus 46 items on which the judges agreed that if marked in one way they would present a point of view consistent with the attitude of an individual favorable to the New Deal, whereas if marked in the opposite way they would be opposed to the attitude of an individual favorable to the New Deal.

The passage upon which the items were based was now revised so that 23, or half of the items, would be answered in the passage in the same way that the judges had answered them, and so that the other 23 would be answered contrary to the judges' answers.[3] Consequently the test items were based on a passage which answered half of them in a manner favorable to the New Deal and half in a manner unfavorable. The test is in reality 2 tests, each consisting of 23 items. For convenience, one of them may be called the Pro New Deal and the other the Anti New Deal test.

The rating scale. A 7-point rating scale was constructed to obtain ratings from the subjects of apparent attitude toward the New Deal of each of the following: (a) content of the passage; (b) the speaker who read the passage. Ratings on the same scale were also obtained for the attitudes toward the New Deal of: (a) the subjects previous to hearing the passage; (b) the subjects after hearing the passage; (c) the subjects' mothers; (d) the subjects' fathers.

Collection of the data. In chronological order the various steps involved in collecting the data were:

1. The subjects were read the following instructions:

As you know, 1940 is an election year, and the radio promises to be a very important medium for presenting political issues to the American voters.

[3] The items were divided between the two points of view on a chance basis.

Psychologists are particularly interested in the part that radio plays in elections; yet there are many questions which we must be able to answer if we are to understand its influence. One of these is the amount which the average listener remembers when he hears a radio speech, and that is the question which we are going to ask you to help us to answer today.

I am going to read a 10-minute political speech to you, and after I have finished I am going to give you a test to find out *how much of the speech you remember.* Now please do not take any notes while I am reading the speech. Just try and listen attentively in much the same manner that you would ordinarily listen to a radio speech in which you were interested. Are you ready? Then I shall begin.

2. The speech was then read.

3. The rating scale was filled out by the subjects.

4. As soon as the subjects had finished with the rating scale, they were given the recognition test. *It was again stressed by the experimenter that all of the questions were to be answered solely on the basis of the material contained in the speech.*

5. Twenty-one days later and without prior warning the subjects were again tested, and a new rating of their attitudes toward the New Deal was obtained.

The experimental groups. On the basis of initial attitude toward the New Deal (attitude previous to the speech) 3 experimental groups with 48 subjects in each group were selected for further investigation.[4] Table 1 describes each of the groups in terms of their mean ratings on the scales mentioned earlier. *The significant differences between these groups for the purposes of this study is their attitude toward the New Deal.* One group is definitely favorable to the New Deal, one group is neutral, and one group is definitely opposed to the New Deal. The groups were so selected that there was no overlapping of their ratings of attitude toward the New Deal. Indeed, as Table 1 will show, the groups are fairly well separated on most of the other factors which they rated. Standard errors of the mean ratings for each group are not presented because they would be relatively meaningless applied to distributions of ratings which fall on 1 or 2 points of the scale only.

It is interesting to note that the speech resulted in little or no change in the subjects' attitudes toward the New Deal. The favorable group is still favorable, the neutral group is still

[4] The size of the groups was determined by the number of subjects who expressed strong approval of the New Deal. Out of 321 students tested, only 48 expressed a strong preference for the New Deal.

neutral, and the opposed group is still opposed to the New Deal. In fact, there is very little apparent change in attitude even after a period of 3 weeks. That the frame of reference is influential in the subjects' interpretations of the attitude expressed by the speaker and by the content of the speech is evidenced by the mean ratings of the 3 groups on these factors. The reactions of the various groups was in all cases consistent with their frame of reference—*this despite the fact that all of the subjects heard the speech and the speaker at the same time and under the same conditions.* It should also be remembered that the speech in reality was neutral in so far as it contained an equal number of favorable and unfavorable statements about the New Deal.

TABLE 1

MEAN RATINGS OF ATTITUDE TOWARD THE NEW DEAL OF EACH OF THE EXPERIMENTAL GROUPS ON SEVEN FACTORS *

(N in each group = 48)

FACTORS RATED	EXPERIMENTAL GROUPS		
	FAVORABLE	NEUTRAL	UNFAVORABLE
1. Own attitude previous to speech	1.79	4.08	6.35
2. Own attitude immediately after speech	1.79	4.23	6.08
3. Own attitude 3 weeks after speech	2.42	4.08	6.06
4. Attitude represented by speech	3.53	3.92	5.48
5. Attitude represented by speaker	3.11	3.63	4.25
6. Estimate of mother's attitude	2.87	4.17	5.91
7. Estimate of father's attitude	2.82	4.30	6.36

* 1 = Very Favorable
2 = Favorable
3 = Slightly Favorable
4 = Neutral
5 = Slightly Unfavorable
6 = Unfavorable
7 = Very Favorable

Analysis of variance.[5] The experiment was so designed that it would permit application of R. A. Fisher's analysis of variance (8). Analysis of variance is fundamentally a test of significance, but with this additional advantage. It enables one to divide the total variation of a body of data into several parts traceable to specific sources. Where it is impossible to place

[5] The writer wishes to express his indebtedness to Dr. L. G. Humphreys for critical assistance in the preparation of the factorial design and for invaluable discussions of the application of analysis of variance to psychological problems.

important factors under direct experimental control, but where we still wish to observe their influence, we may resort to statistical control. If we desire to study two or more factors in the same experiment, analysis of variance enables us to do so efficiently, this being especially true if the factors are varied in more than two ways. In contrast to this procedure, the older statistical methods are well adapted to handle problems where only one factor is varied in two ways.

Another advantage of analysis of variance is that it offers the investigator an opportunity to discover the significance of the *interaction* between his different factors. As Woodworth (**14**) remarks: "With careful planning two or three independent variables can sometimes be handled in a single experiment with economy of effort and with some chance of discovering the interaction of the two or more factors." A significant interaction variance in the design of our experiment would show that the effect of a certain attitude is not always the same but depends upon the type of learning material to which it is applied.

The steps in the computation of analysis of variance are described in detail by Fisher (**7**) and Snedecor (**11**; **12**). We may briefly summarize them, following the outline given by Snedecor (**12**), in order to give a more complete understanding of the technique and a consequent frame of reference for the discussion of results which follows. (a) The total sum of squares of deviations from the general mean is found. This is known as the *total variance.* (b) The total variance is then broken down into specific parts, each of which is traceable to one of the experimental factors. (c) The *error variance* (variance which cannot be accounted for by the experimental factors) is then found by subtracting the sum of the variances of the different experimental factors from the total variance. Error variance evaluates those sources of variation which are outside the controls of the experiment (in our case such things as motivation, fatigue, individual differences, etc.) and which are properly assigned to experimental error. (d) The different sums of squares are now divided by their appropriate degrees of freedom to give the *mean square or variance* for each factor. (e) A test for significance of each of the factors is provided by *F*. *F* is the ratio of the mean square of the factor under con-

sideration to the mean square of error. A table of significant values ($P = .01$ and $.05$) of F for appropriate degrees of freedom is given by Snedecor (12).

The factorial design. The design of the experiment involved 3 important factors: (a) initial attitude; (b) type of learning material; (c) time. Only 2 of the factors, however (each varying in a specific number of ways), shall be considered at any one time. Attitude is varied in 3 ways by using 3 experimental groups: (a) Favorable; (b) Neutral; (c) Unfavorable. The learning material is varied in 2 ways: (a) Pro New Deal; (b) Anti New Deal. Time is varied in 2 ways by giving the recognition test once immediately after reading the speech and once 21 days later: (a) Immediate; (b) Delayed. By varying these factors systematically in the experiment, 4 important analyses of the data are possible. Each of these will be discussed in turn.

RESULTS AND DISCUSSION

Analysis 1. The first analysis is for the purpose of determining whether attitude is an important factor in immediate recognition, as evidenced by responses on the test immediately following the reading of the passage. More specifically, we are interested in knowing whether a greater frequency of correct responses on the Pro New Deal test will be associated with a favorable attitude toward the New Deal, and whether a greater frequency of correct responses on the Anti New Deal test will be associated with an unfavorable attitude.

Figure 1 gives a graphic presentation of the sum of correct responses on the Pro and Anti New Deal material for each of the experimental groups. The graph shows that the number of correct responses on each test is directly related to attitude. Note also the very slight discrepancy in the responses of the neutral group on the two tests as compared with the other groups. With improved measuring instruments, one would expect the two graphs to form a perfect X, crossing directly at the point where the neutral group is represented.

Table 2 presents the figures upon which the graphs in Figure 1 are based. The group favorable to the New Deal makes a slightly larger number of *total* correct responses than do the

other groups; if we assume that the New Deal represents a slightly more "radical" political position, then this fact is consistent with the positive correlations which have been reported between ability and "radicalism."

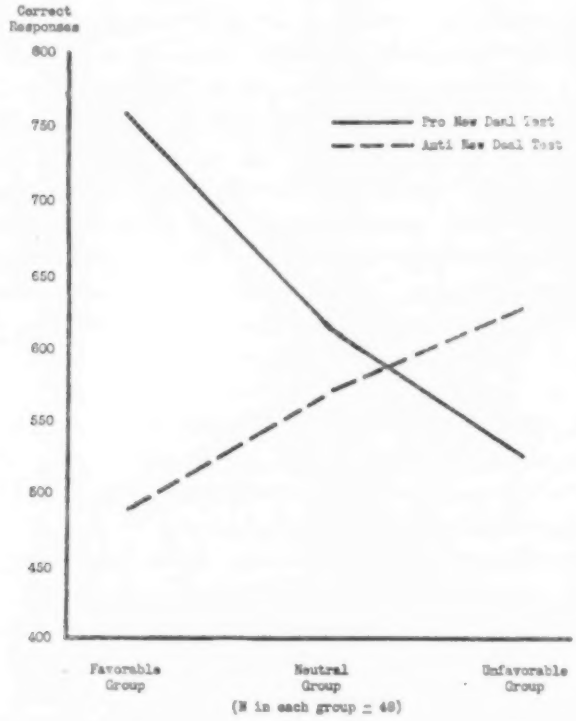

FIGURE 1. A GRAPHIC PRESENTATION OF THE SUM OF CORRECT PRO AND ANTI RESPONSES ON THE IMMEDIATE TEST FOR EACH OF THE EXPERIMENTAL GROUPS

TABLE 2

THE SUM OF CORRECT PRO AND ANTI RESPONSES ON THE IMMEDIATE TEST FOR EACH OF THE EXPERIMENTAL GROUPS

IMMEDIATE TEST	ATTITUDE			TOTAL
	FAVORABLE	NEUTRAL	UNFAVORABLE	
Correct Pro Responses	755	616	523	1894
Correct Anti Responses	477	565	625	1667
Total	1232	1181	1148	3561

The analysis of variance of the data is presented in Table 3. The *F* values which are starred may be regarded as significant in so far as these values would occur by chance in less than 1 per cent of all random samples. The significant test variance merely indicates that the Pro New Deal test is relatively easier than the Anti New Deal test. This fact does not, however, invalidate our results; it is simply another instance where analysis of variance proves to be a superior technique when compared to the more conventional statistical methods. The differential difficulty of the two tests has been adequately controlled by application of analysis of variance.[6] That is to say, knowing the amount of variance which can be attributed to the tests themselves, we have eliminated it from our estimate of error.

TABLE 3

ANALYSIS OF VARIANCE OF CORRECT PRO AND ANTI RESPONSES ON THE IMMEDIATE TEST

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	*F*-VALUE	1 PER CENT POINT
Error	282	3510.90	12.45		
Test	1	178.92	178.92	14.37**	6.73
Attitude	2	37.31	18.66	1.50	4.68
Interaction	2	761.59	380.80	30.59**	4.68
Total *	287	4488.72	15.65		

* Note that the *addition theorem* applies to the degrees of freedom and sum of squares. It provides an accurate check on some of the calculations involved in analysis of variance.
** Significant variance (P less than .01).

The striking feature of the analysis is the highly significant interaction variance. This may be interpreted as meaning that the attitudes which were associated with each of the 3 groups produced correct responses on one of the tests which were out of accord with their correct responses on the other. Thus the effect of a certain attitude is not always the same but depends upon the type of material to which it is applied. Individuals with favorable attitudes toward the New Deal tend to make more correct responses on the Pro New Deal test than on the Anti New Deal test, whereas individuals with unfavorable attitudes toward the New Deal tend to make just the reverse.

[6] For a critical discussion of the factor of differential difficulty involved in using 2 different tests of learning, see Edwards and English (5; 6).

This fact is of course consistent with the hypothesis advanced earlier. Material which harmonizes—which is readily assimilated—with respect to an existing frame of reference is learned significantly better than material which conflicts with—which is opposed to—an existing frame of reference. That this is not an artifact of the material as such is evidenced by the fact that *the very same material which is learned by one group is not learned by another, while the material which is not learned by the first group is learned by the second.*

Analysis 2. In the second analysis we shall determine whether attitude is still a dominant influence associated with correct responses after a lapse of time. From Table 4 we see the same

TABLE 4

THE SUM OF CORRECT PRO AND ANTI RESPONSES ON THE DELAYED TEST FOR EACH OF THE EXPERIMENTAL GROUPS

DELAYED TEST	ATTITUDE			TOTAL
	FAVORABLE	NEUTRAL	UNFAVORABLE	
Correct Pro Responses	695	552	457	1704
Correct Anti Responses	370	455	565	1390
Total	1065	1007	1022	3094

trend present at the time of the delayed testing that was present at the time of the immediate test. The effect of attitude is indicated by the definite decrease in the number of correct responses on the Pro New Deal test from the favorable to the unfavorable group, and by the definite increase on the Anti New Deal test. A certain amount of forgetting between the first and second tests is evidenced by the general decrease in the sums of correct responses on the delayed test (Table 4) when compared to the immediate test (Table 2).

The analysis of variance of these data with the significant values of F starred is presented in Table 5. The statistically reliable factors present at the time of the delayed test are the same as those present at the time of the immediate test. Thus the same conclusions made concerning the relationship between attitude and immediate responses hold for delayed responses as well. It would appear that attitude is not only a determining

factor in immediate recognition but that its effects remain over a period of time.

Analysis 3. The present analysis is primarily concerned with the effect of time on the retention of correct responses to the items appearing on the Pro New Deal test. The sums pre-

TABLE 5

ANALYSIS OF VARIANCE OF CORRECT PRO AND ANTI RESPONSES ON THE DELAYED TEST

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	*F*-VALUE	1 PER CENT POINT
Error	282	4870.33	17.27		
Test	1	342.35	342.35	19.82**	6.73
Attitude	2	18.89	9.45	.55	4.68
Interaction	2	977.42	488.71	28.30**	4.68
Total	287	6208.99	21.53		

** Significant variance (P less than .01).

sented in Table 6 indicate that there is a general decrease in the number of correct responses from the first to the second test for each group. The difference between the number of correct responses on the immediate and delayed tests for each group may be expressed in terms of the percentage forgotten. The figures for the favorable, neutral, and unfavorable groups are 8, 10, and 13 per cent, respectively. These figures indicate that there is a slight tendency for the relative amount of forgetting on the Pro New Deal test to increase as attitude toward the New Deal becomes more unfavorable.

TABLE 6

THE SUM OF CORRECT PRO RESPONSES ON THE IMMEDIATE AND DELAYED TEST FOR EACH OF THE EXPERIMENTAL GROUPS

PRO TEST	ATTITUDE			TOTAL
	FAVORABLE	NEUTRAL	UNFAVORABLE	
Correct Immediate Responses	755	616	523	1894
Correct Delayed Responses	695	552	457	1704
Total	1450	1168	980	3598

The analysis of variance of these data is presented in Table 7. The starred values of *F* are highly significant. The time variance indicates that the amount forgotten from the first to the second test is reliable. The significant attitude variance informs us that the sums of the immediate plus the delayed responses differ reliably from group to group. The fact that the interaction variance is not significant is to be interpreted as meaning that the tendency noted for the relative amount forgotten to

TABLE 7

ANALYSIS OF VARIANCE OF CORRECT PRO RESPONSES ON THE IMMEDIATE AND DELAYED TEST

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	*F*-VALUE	1 PER CENT POINT
Error	282	3640.58	12.91		
Time	1	125.35	125.35	9.71**	6.73
Attitude	2	1165.87	582.94	45.15**	4.68
Interaction	2	.19	.10	.01	4.68
Total	287	4931.99	17.18		

** Significant variance (P less than .01).

increase as attitude toward the New Deal becomes more unfavorable is thus not large enough to meet the requirements of statistical significance.

Analysis 4. This analysis is a duplication of the previous analysis with the exception that here we are concerned with the retention of correct responses on the Anti New Deal test as related to time and attitude. From the sums presented in Table 8 we may again compare the relative number of correct

TABLE 8

THE SUM OF CORRECT ANTI RESPONSES ON THE IMMEDIATE AND DELAYED TEST FOR EACH OF THE EXPERIMENTAL GROUPS

ANTI TEST	ATTITUDE			TOTAL
	FAVORABLE	NEUTRAL	UNFAVORABLE	
Correct Immediate Responses	477	565	625	1667
Correct Delayed Responses	370	455	565	1390
Total	847	1020	1190	3057

responses forgotten between the time of the immediate and delayed tests. The percentage forgotten for the favorable, neutral, and unfavorable groups is 22, 19, and 10 per cent, respectively. There is thus a slight tendency for the relative amount forgotten on the Anti New Deal test to increase as attitude toward the New Deal becomes more favorable. This trend is just the opposite of that found for the Pro New Deal material. However, when we consider both trends in terms of the frame of reference involved, we find a very consistent picture. In both cases the relative amount forgotten increases as the *degree* of conflict between the material and attitude increases.

The analysis of variance of these data with the significant *F* values starred is presented in Table 9. Time exerts the same

TABLE 9

ANALYSIS OF VARIANCE OF CORRECT ANTI RESPONSES ON THE IMMEDIATE AND DELAYED TEST

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	*F*-VALUE	1 PER CENT POINT
Error	282	4740.65	16.81		
Time	1	266.42	266.42	15.85**	6.73
Attitude	2	612.77	306.40	18.23**	4.68
Interaction	2	16.38	8.19	.49	4.68
Total	287	5636.22	19.64		

** Significant variance (P less than .01).

influence on the Anti New Deal responses as it does on the Pro New Deal responses, i.e., significant forgetting takes place between the immediate and delayed test. The reliable attitude variance indicates that the total sum of correct responses on the immediate plus the delayed test is greater for the group which is unfavorable to the New Deal than for the others. The lack of any significant interaction variance again informs us that the observed tendency for the relative amount forgotten to be related to attitude is not large enough to be considered reliable.

SUMMARY AND CONCLUSIONS

It is possible to view the retention of experiences from everyday life in terms of the relationship between these experiences and the frame of reference of the individual involved, rather than in terms of the pleasantness or unpleasantness of the experiences. Frame of reference is a concept which is used to designate the organization of desires, attitudes, wishes, values, etc., within an individual as a result of learning. We have proposed that experiences which are in harmony with an existing frame of reference will tend to be learned and remembered better than experiences which are in conflict with the same frame of reference.

In order to investigate this hypothesis, an experiment was designed which made use of analysis of variance. Three groups of subjects, with 48 subjects in each group, were used. One group was favorable, one was neutral, and one was unfavorable with respect to attitude toward the New Deal. All 3 groups heard a 10-minute speech concerning the New Deal. Half of the material in the speech was designed to present a point of view favorable to the New Deal and half a point of view decidedly unfavorable. The subjects were not aware of the true purpose of the experiment, but were under the impression that only their ability to remember (independent of any consideration of attitude) was being studied.

Immediately after hearing the speech, the subjects were tested for recognition of the material presented. Twenty-one days later and without prior warning they were re-tested. The recognition test used in the experiment consisted of an equal number of items on the favorable and unfavorable parts of the speech. One half of the test thus measured recognition of the Pro New Deal material and one half measured recognition of the Anti New Deal material. The advantage of this technique is obvious. The same material is heard by all of the subjects under the same conditions and at the same time, the only variable being that of attitude.

According to our hypothesis, the material favorable to the New Deal should be readily assimilated by individuals with favorable attitudes toward the New Deal, while for individuals

with unfavorable attitudes the same material should involve a conflict. Likewise the material unfavorable to the New Deal should involve a conflict for those individuals with favorable attitudes toward the New Deal, but should be readily assimilated by individuals with unfavorable attitudes. We thus have measures of recognition of both conflicting and harmonizing material for both individuals with favorable and unfavorable attitudes toward the New Deal.

The results of the analysis of our data may be briefly summarized by the following conclusions:

1. The political frame of reference of our subjects was a determining factor in their judgments of (a) the manner in which the speaker presented a "neutral" speech on a controversial subject pertinent to that frame and (b) the point of view expressed by the content of his speech. That is, subjects with favorable attitudes toward the New Deal regarded both the speaker and the content of his speech as favorable to the New Deal, whereas subjects with unfavorable attitudes regarded them as unfavorable.

2. The political frame of reference was relatively stable over the period of the experiment (21 days) despite the fact that during this period many important events took place which might have reorganized the frame. (The subjects first heard the passage on February 14, 1940, and were tested immediately after the reading. The second testing was on March 6.)

3. Recognition of material which harmonized with the frames of reference of our subjects was significantly better than recognition of material which conflicted with the same frame of reference. Furthermore, the amount recognized was directly related to the degree of harmony of the material with respect to the frame of reference.

4. Significant forgetting of both conflicting and harmonizing material occurred between the immediate and delayed test (21 days). There was, however, a consistent but statistically unreliable tendency for the relative amount forgotten to be related to the degree of conflict between the material and the frame of reference.

These conclusions, in general, would seem to support the hypothesis advanced earlier.

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THE COMPARATIVE FATIGABILITY OF NORMAL AND MENTALLY DEFICIENT CHILDREN

BY HARRIETT L. VALENTINER
Cincinnati

IT has been observed that mentally deficient children cannot profitably be kept at certain types of classroom activities for as long a time as normal children, even though the work is planned to meet their level of intelligence. With a view to discovering the cause of this lowered ability to continue with certain types of school activities and the seeming lack of endurance manifested by the mentally deficient, the present study was undertaken. A search of available literature disclosed the fact that no such study involving a number of tests of fatigue in mental and physical tasks, given under rigidly controlled conditions, has been attempted with individuals of equal chronological ages within these two groups.

SUBJECTS AND CONDITIONS

The subjects secured for this study were divided into two groups; namely, the experimental group, composed of mentally deficient children from Seguin School, the central special school for mentally deficient children in Cincinnati; and the control group, composed of normal children from Webster School, a normal public school in Cincinnati. Both of the schools were in the same downtown school area, on the same street and within five city blocks of each other. Most of the children attending the schools lived within walking distance and went home for their noon meal. Since Seguin is not an institution, but a public school, all of the subjects went to their respective homes at night. The children from Seguin may have had a slight advantage in that their school day is not as long as that of the normal school. It begins one-half an hour later than Webster, and is dismissed fifteen minutes earlier.

Since the children in both groups lived in the same downtown area (Webster school children were particularly chosen because

of this similarity), it was assumed that local environmental conditions were as identical as it was possible to make them. Home conditions, social and economic conditions were also compared and a remarkable similarity found. Some children attending Webster had a brother or sister attending Seguin. On taking the children home after the testing it was discovered that some of the normal children lived in the same tenement houses as did the mentally deficient children and that others of both groups were close neighbors and playmates. Economic status appeared to average the same, judging from the location and condition of the homes, and it was felt that a rather high degree of standardization had been secured as far as environment and home conditions were concerned.

The narrowed basis of selection of pupils from Webster school was that of an I.Q. rating secured from a Kuhlmann-Anderson Fourth Grade Test given by members of the Psychological Department of the Cincinnati Public Schools. Only those with an I.Q. within the limits of 91–111 were considered as prospective subjects.

The chronological ages of children at Seguin were matched with the prospective subjects from Webster, and children having the same chronological age were selected to be tested with a normal competitor. These children from Seguin were then given the Kuhlmann-Anderson Fourth Grade Test and their I.Q.'s computed. Children unable to read were excluded from the group taking this test.

A very careful checking and pairing made it possible to match normal and mentally deficient children so closely that no normal child was more than a month older or younger than his mentally deficient partner. Tabulation of ages revealed the extraordinary fact that one normal and one mentally deficient boy selected were born on the same day; that nine pairs had birth dates within five days of their partners; that seven pairs had birth dates within ten days of their partners; and that the remaining nine pairs had birth dates within twenty days of their partners.

Since it is doubtful whether a closer pairing of age and sex could be accomplished, the experimenter considered it fortunate that twenty-six such closely matched pairs of children were available for testing. Other subjects with chronological ages farther

apart or of opposite sex could have been used; but in view of the fact that the experiment aimed to make as exact measurements as possible, only these twenty-six pairs were tested.

Seventeen pairs of boys and nine pairs of girls then became the subjects for the fatigue tests. The age range of both groups was from 10 years and 3 months to 13 years and 4 months. The median age of the mentally deficient or experimental group was 12 years and one-half month. The median age of the normal or control group was 12 years. The I.Q. range of the experimental group was from 50 to 76, with the mean 65 and the median 65.5. The I.Q. range of the control group was from 91 to 111 with the mean 100 and the median 100.5.

Tests in which all subjects had an equal opportunity were selected. The mental tasks required only an ability to recognize the numerals from 1 to 9, the letters of the alphabet, and four common colors plus white. This ability was determined before the tests were administered. The physical tasks were equally devoid of difficult requirements and demanded only attention to simple instructions.

PROCEDURE

All subjects were tested two at a time after school at the Psychological Laboratory of the University of Cincinnati. Rapport was established between the experimenter and the subjects on the way to the laboratory, so that the subjects were at ease when the tests began.

All subjects performed under approximately the same conditions. Physical conditions of the laboratory were kept constant as to light, temperature, and equipment. The subjects were given identical instructions by the experimenter. Some of the tests were administered to the two subjects at the same time, while in other cases one subject preceded his partner by a short period. The only observer present was the head of the department, who assisted with the color-naming test.

A group of tests calculated to measure best the amount and rate of fatigue was given to all subjects. This group included number and letter cancellation, ergographic performance, and color naming. Two trials of steadiness were given in order to determine the difference in steadiness of the two groups and the

effect of fatigue on steadiness. Strength of hand grip was determined before and after the administering of the steadiness and fatigue tests.

RESULTS

Computed results of all tests show several significant differences between the two groups.

1. *Strength of grip.* In the tabulation of results, the relation of the initial and final strengths of grip of the normal and mentally deficient groups is quite evident. The normal group had an initial average squeeze of 15.61 kg. and a final average squeeze of 14.88 kg., indicating a drop of .73 kg. The mentally deficient group had an initial average squeeze of 13.88 kg. and a final average squeeze of 13.03 kg., indicating a drop of .85 kg. Decrement was shown in the strength of grip of both the normal and mentally deficient groups, with the mentally deficient girls showing the greatest decrease. Nine normal and sixteen mentally deficient subjects decreased, but the average decrements were not enough to be significant as far as fatigue is concerned. The findings do agree, however, with previous findings of other examiners in regard to the relation of strength of hand to mental ability. Smedley, Schuyten, and Carman found evidences of positive correlation in this respect. Barr also found that hand grasp and mental grasp go hand in hand (2). In this present study of fifty-two children, the normal group exceeded the mentally deficient group by 1.73 kg. in the first test and 1.88 kg. in the second. Findings also agree with those of previous examiners who found that boys are uniformly stronger than girls (2). In this test, the boys exceeded the girls by an average of 4.72 kg. in the first test and 5.72 kg. in the second.

2. *Number cancellation.* Results are significant in that they show a greater decrease in the number examined and number correct for the mentally deficient group than for the normal group. The normal group examined an average of 50.5 numbers in the first minute and an average of 47 numbers in the last minute, a drop of 3.5. The mentally deficient group examined an average of 37 numbers in the first minute and an average of 26.5 numbers in the last minute, a drop of 10.5. The normal group showed 49 as the average number correctly cancelled in

the first minute, and 46 as the average number in the last minute, a drop of only 3. The mentally deficient group showed 31 as the average number correctly cancelled in the first minute, and 23 as the average number in the last minute, a drop of 8. In computing the average ratio of the number of errors to the number examined by each group, it was found that the normal group had a ratio of .03 in the first minute and .02 in the last minute. For the mentally deficient group the ratio was .15 in the first minute and .17 in the last minute.

3. *Letter cancellation.* In letter cancellation even more significant differences appear. The normal group examined an average of 45 letters in the first minute and an average of 44 letters in the last minute. The mentally deficient group examined an average of 36.5 letters in the first minute and an average of 21 letters in the last minute, a drop of 15.5. The normal group showed an average of 43.5 letters correctly cancelled in the first minute and an average of 43 letters correctly cancelled in the last minute. The mentally deficient group showed an average of 31.5 letters correctly cancelled in the first minute and an average of 18 letters in the last minute, a drop of 13.5. In computing the average ratio of the number of errors to the number examined by both groups, it was found that the normal group had an average ratio of .04 in the first minute and .03 in the last minute. The mentally deficient group had an average ratio of .14 in the first minute and .24 in the last minute, an increase of .10. Eleven of the mentally deficient subjects stopped cancelling before the last minute, while six of the normal group completed the work before the last minute. In both number and letter cancellation all normal subjects fatigued less than their mentally deficient partners, with the exception of five cases. In two of these cases a mentally deficient boy and a mentally deficient girl showed as little fatigue as did their normal partners, in both number and letter cancellation. In the cases of the other three, all in number cancellation, the normal subjects showed as much fatigue as did their mentally deficient partners. The evidences of fatigue in letter cancellation support the conclusion drawn as a result of the number-cancellation test, namely, that in cancellation the mentally deficient group of subjects tires more than the normal group.

4. *Steadiness tests.* In the tabulation of results, the difference between the first and second steadiness tests of the normal and mentally deficient groups is readily observed. The normal group registered 6.54 as the average number of contacts in the first test, and 5.65 as the average number in the second test. The mentally deficient group registered 13.19 as the average number of contacts in the first test and 11.84 as the average number in the second test. Both groups showed an increase in steadiness, the normal a gain of .89 and the mentally deficient group a more considerable gain of 1.35. The mentally deficient girls showed the greatest increase in steadiness.

Only one interpretation can be placed on these results. The first steadiness test, coming as it did after the combined twenty-seven minute cancellation test, involved the use of the same two fingers to hold the stylus that held the pencil in cancelling. It was observed that there is a tendency for many subjects to grip the pencil more tightly as cancellation becomes more difficult and the performance more forced. It is certainly possible that this tightening of the thumb and index finger muscles produced a tenseness responsible for the loss of steadiness in the first test. The gain in steadiness in the second test seems to confirm the theory that there is little transference of fatigue from one muscle to another. In the ergograph test the third finger was used in performing the task; consequently the fatigue did not affect the fingers which held the stylus. These fingers had a rest of about fifty minutes during the color naming and ergographic tests—a rest sufficient to produce a contrast in the steadiness shown immediately after cancellation and that shown after the performance on the ergograph. The larger gain of 1.35 by the mentally deficient group can be explained by the fact that this group fatigues the most in cancellation and consequently was the most tired when the initial steadiness test was given. As a test of the influence of the ergograph on general fatigue, the results are worthless, but as a test of the transfer of fatigue where identical elements are concerned, the unexpected results are of some value.

5. *Color naming.* The results of the color-naming test are significant in many respects.

(a) *Responses.* The normal group averaged 55 responses in

the first minute and 45 responses in the last minute. The mentally deficient group averaged 38 responses in the first minute and 30 responses in the last minute. Results reveal that although the level of performance was lower throughout for the mentally deficient group, there is no difference between the two groups in the amount of fatigue as indicated by the decrement in response.

(b) *Errors.* The normal group had an average error of .84 in the first minute and one of .96 in the last minute. The mentally deficient group had an average error of 1.84 in the first minute and one of 2.28 in the last minute. This finding indicates that though the responses of the two groups showed the same decrease in number, the errors of the mentally deficient group increased as the performance continued, due to the onset of fatigue.

(c) *Blocks.* The term "block" refers to those periods experienced by mental workers when they are unable to respond and cannot continue until after the lapse of a short time. The "block" is indicated by a pause in the responses equivalent to the length of two or more average responses. In defining the block in this way instead of by a constant unit of time, it is possible to make the criterion relative to the speed of the subject. It has been found by Bills (1) that the frequency and length of these blocks is a much more sensitive indicator of the onset of fatigue than the frequency of responses. For the normal group the average number of blocks was 3.4 in the first minute, and 3.7 in the last minute, an increase of only .3. For the mentally deficient group the average number of blocks was 4.5 in the first minute, and 7.2 in the last minute, an increase of 2.7. From these results we may infer that there was during the performance of the mentally deficient group an early and continuous increase in the frequency of the involuntary pauses or "blocks" which was not present in the performance of the normal group. These pauses indicate that it was more necessary for the mentally deficient group to rest in order to ward off fatigue than it was for the normal group.

(d) *Total block time.* The total length of blocks for each group for each minute was measured and recorded. The normal group showed a total block time of 43 one-sixth of seconds in the first minute and a total block time of 58 one-sixth of seconds

in the last minute, an increase of only 15. The mentally deficient group showed a total block time of 81 one-sixth of seconds in the first minute and a total block time of 138 one-sixth of seconds in the last minute, an increase of 57. The normal group consumed less than one-sixth of the last minute in blocking, whereas the mentally deficient group consumed more than one-third of the last minute in blocking.

(e) *Average block time.* The difference between the two groups in the average block time of each minute was also calculated. The average block time for the normal group was 12.3 one-sixth of seconds in the first minute, and 11.8 one-sixth of seconds in the last, an increase of only .5. The average block time for the mentally deficient group was 17.3 one-sixth of seconds in the first minute, and 19.7 one-sixth of seconds in the last minute, an increase of 2.4. Since average block time is based on the total block time divided by the number of blocks which made that time, the results indicate the same trend. Average block time was greater for the mentally deficient group than for the normal group and showed an earlier increase.

Combining the results of all the factors measured in color naming, particularly those of "blocks," only one inference can be drawn; that is, that in color naming, mentally deficient children fatigue earlier, more rapidly, and to a greater extent than do normal children. Examination of individual records showed that with the exception of six cases all mentally deficient subjects tired more than their normal partners.

6. *Ergographic performance.* Results in rate of performance on the ergograph show that both the normal and the mentally deficient groups tired considerably when their final averaged points on the work curve were compared with their initial points. The curves of both groups descended side by side, ending within .5 inch of each other. This circumstance leads one to believe that on ergographic performance there is no difference between the two groups in the tendency to fatigue. Individual records showed a higher level of performance and less fatigue for the normal subjects in all cases except ten. In three of these cases the mentally deficient subjects performed higher and fatigued less than their normal partners, and in the other seven cases the mentally deficient subjects per-

formed at a lower level but showed no more fatigue than their normal partners. Examination of results showed that a number of both normal and mentally deficient subjects began to drop out after seventy-five pulls. These subjects thenceforth were given a score of zero. This fact accounts in part for the final close parallelism of the two curves when the total pulls for each group were averaged.

Statistical computation to determine the reliability of differences between some of the average work decrements of the normal and mentally deficient subjects indicates that the results are significant. This computation includes average decrements or increments, probable errors, probable errors of differences, and coefficients of reliability.

SUMMARY OF RESULTS

When the results are viewed as a whole, they show a rather clear differentiation between those relating to physical stamina and muscular fatigue, and those relating to mental performance level and mental fatigability.

On the side of physical performance, although the mentally deficient subjects showed a somewhat reduced strength of grip at the outset, nevertheless they suffered no greater decrement in grip strength due to fatigue than did the normal subjects. Likewise, in the erographic performance, they gave no indication of a more rapid drop in output than the normal subjects. They were somewhat inferior on the steadiness tests.

On the side of mental performance, on the other hand, the mentally deficient subjects were clearly inferior to the normal in both accuracy and amount accomplished. This fact showed both in the general level of their performance and in the steepness of the fatigue increment in their work curves. It was most apparent in the color-naming and letter-cancellation tasks, and least apparent in the cancellation of numbers.

CONCLUSIONS

The following general conclusions from this study are justified both in terms of the differences in average results for the control and experimental groups, and in terms of the very small

number of individuals in the respective groups who showed tendencies contradictory to the average trends.

1. Mentally deficient children, in terms of the I.Q. levels herein studied, are somewhat inferior to normals in muscular strength, as indicated by a weaker hand grip and somewhat lower steadiness, but they are not markedly more susceptible to muscular fatigue resulting from continuous ergographic performance.

2. In mental work, such as color-naming and cancellation, mentally deficient children perform at a somewhat lower level and with less accuracy than do normal children.

3. The amount and rate of onset of fatigue in such mental work is greater in the case of the mentally deficient, as is indicated by the greater decrement in their speed and accuracy and the greater accumulation of blocks.

DISCUSSION

The fact that the mentally deficient tire more readily than the normal in predominantly mental work has some bearing on our knowledge of the I.Q. In all probability there is a tendency to fatigue which must be distinguished from the ability to perform. It is presumably a distinct dimension of intelligence which nevertheless correlates roughly with the I.Q. The correlation is not a perfect one perhaps, but there is reason to believe that fatigue engendered in work predominantly mental does vary with the level of intelligence. That fatigue is a distinct dimension of intelligence is further suggested by the fact that mere ability to carry out the tasks set was not a matter of level of intelligence, inasmuch as they were within the capability of all subjects, including the one bordering on imbecility (I.Q. 50).

The fact that the mentally deficient fatigue more rapidly and to a greater extent in mental work than do the normal suggests that further checks on the relation of fatigue to intelligence might be undertaken.

It might be well to pursue a study of the higher levels of intelligence for the purpose of discovering whether such individuals show less fatigue than those on the lower levels.

It would also be interesting to know whether the resistance

to mental fatigue increases with growth of intelligence and age. Winch (3) intimates that such appears to be the case.

The fact that the tasks selected for use in this study were of the very simplest type suggests that the use of tasks of a more complex nature might yield different results. If, however, a study should be made in which more complex tasks were used, care should be exercised to select tasks not too complex for mentally deficient subjects to handle.

The fact that in the measurement of muscular fatigue the tests were confined to only a small part of the musculature suggests that, if the entire body were put to a test, considerably greater difference between normal and mentally deficient individuals would be found. Manifestations of such an apparent difference have been observed during the participation of the two groups in competitive sports. Here the mentally deficient break down in the final minutes of play. Such a collapse could probably be attributed to a difference in volitional control, which is in itself a mental attribute rather than a muscular weakness; but how much of it is fatigue is a problem worth pursuing.

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THE PRODUCTION OF BLISTERS BY HYPNOTIC SUGGESTION: A REVIEW

BY FRANK A. PATTIE, JR.
The Rice Institute

CAN organic functions which are not under voluntary control be modified or controlled by hypnotic suggestion? This fundamental question must be answered by every author of a theory of hypnosis.

Some of the experiments and clinical observations purporting to show that certain functions (such as those of the digestive glands and the kidneys) not under voluntary control can be modified in the trance are discussed by Hull (**10**). In discussing the production of gastric secretion when hallucinations of eating are suggested, he points out that certain other experiments show similar reactions in the waking state if the subjects merely talk about eating. The gastric secretions, in other words, are actually under indirect voluntary control. Hull also reviews an experiment in which herpetic blisters (cold sores) were produced in the hypnotic state by suggesting (1) the itching sensation which usually precedes the appearance of a cold sore and (2) an extremely unpleasant emotional experience. Inasmuch as the suggestion of itching was not sufficient to produce the sore, it may be concluded that the emotional state lowered the physiological resistance of the subjects and that the sores were a direct consequence of this lowering. In fact, the authors of the experiment, by determining their subjects' opsonic index, showed this to be true. It is not difficult for Hull to explain all of the experimental results which he reviews by reference either to the principle of the conditioned reflex or to some intercurrent emotional state. But he omits any reference to reports of cases in which a cold object, applied to the skin with the suggestion that it is hot, has produced a blister. The processes which produce blisters on the skin are certainly not under direct or indirect voluntary control.

There is a widespread skepticism about the possibility of thus producing blisters. This attitude is in part due to the belief

that the subjects in the experiments were not carefully watched between the giving of the suggestion and the exhibition of the blister and in part to a suspicion that they, with the zeal characteristic of hypnotized and hysterical subjects to give the experimenter the results desired, made the blisters by actually injuring their skin. If this skepticism can be traced to any particular publication, it is most likely due to the article of von Schrenck-Notzing in 1896 (**17**); the author, after having obtained positive results with poor control of his hysterical subject, decided to bandage her arm during the time between the suggestion and the appearance of the blister, whereupon the results were negative and there was evidence that the subject had tried to injure the arm. Another experiment with the arm in a plaster cast was also negative in result.

The following paragraphs contain abstracts, arranged in order of publication of the original articles, of all of the literature that the writer has been able to find on this subject. The abstracts have been made as full as possible with respect to the control of the subjects between the stimulation and the appearance of the skin reaction.

1886. Focachon, a pharmacist who was in touch with Beaunis and Bernheim (**2**; **3**). Subject: a hystero-epileptic woman, 47 years old, whose crises had disappeared under Focachon's hypnotic treatment. The subject was hynotized at 11 a.m. Eight postage stamps were applied on the left shoulder, and she was told that a blistering plaster was being applied. A slight dressing composed of several strips of adhesive plaster and a compress was put over the stamps; its purpose was to impress on the subject the idea that a blister would develop. Subject was watched until night, at which time she was put to bed, still in the trance, and told that she would not awake until 7 a.m. She slept in a room locked by the experimenters. At 8:15 a.m. next morning the dressing and stamps, which were intact, were removed. The place where the stamps were applied was yellowish white in color and surrounded by a zone of redness and swelling about 5 mm. wide. Blisters were not noticed until later on the same day, after subject had made a train trip from Nancy (where the experiment was performed in the presence of Bernheim and others) to her home. Sixteen

days later the skin area was still "en pleine suppuration." Similar stamps applied to the skin of another person for 18 hours produced no irritation.

1888. Von Krafft-Ebing (13). Subject: Ilma S., an hysterical woman used also in Jendrássik's experiments. In the trance, a metal letter K (dimensions not given) was pressed on an area situated somewhat centrally from the left shoulder blade. Suggestion: that next morning the area touched would be blood-red, but there would be no itching. Professor Lipp then bandaged the chest and back so that the area touched was absolutely inaccessible. The bandage was sealed in 4 places. A cover-bandage was then applied and sealed in 2 places. Lipp kept the seal. Next morning Lipp and other physicians examined the bandages and seals, which were intact. On the place touched there was an irregularly shaped area 5.5 by 4 cm. in size. From this spot the epidermis had become detached. The immediate surroundings were red. Two other areas, corresponding to the non-vertical lines of the K, were also present; on them the epidermis was loosened and could be pulled off easily. Nothing is said about control of the subject during the night.

1888. Jendrássik (11). In his best-controlled experiment on Ilma S., Jendrássik states that a blister developed 5 hours after stimulation and that during this interval the subject was watched while she was occupied with some hand work. The blister, however, did not develop where it was supposed to, on the left upper arm, but in a symmetrically located place on the right upper arm. The subject had hysterical right hemianesthesia.

1890. Rybalkin, a student of Bernheim and a physician in St. Petersburg (15). Subject: a 16-year-old boy, a major hysteric with anesthesia of almost the whole body. In the trance, he was told that upon awaking he would accidentally touch a stove (actually cold) and that a burn would result on his arm. Some minutes after contact with the stove there was redness without swelling at the place. A bandage was put on, and the subject went to bed "under the eyes" of the observers. At the end of the sitting, three hours later, there was a considerable swelling, accompanied by redness and a papulous erythema. A bandage was now applied; next morning two blisters were found.

Nothing is said about control during the night, and the paper says no more than is indicated above about the watching of the subject during the sitting of three hours, at which several persons were present.

1906. Doswald and Kreibich (4). The latter author was a distinguished dermatologist and a prolific writer in his field. Experiment A. Subject: Dr. U., a clinical assistant, easily hypnotizable and having a delicate white skin and lively vasomotor reactions. After 3 preliminary experiments, which produced erythemas in 2 cases and nothing in one, the main experiment was done thus. U. was touched lightly on the forearm with a match stick, and the suggestion was given that a blister would form as quickly as possible. After waking U. (apparently immediately afterwards, but the time is not stated), the 2 experimenters watched the place continuously, and no one touched it. After 3 minutes, the place and its immediate surrounding were a delicate rose color, and after another 6 minutes the epidermis had raised itself to form a thin-walled, flabby blister about the size of a bean. After 48 hours, the blister was excised without tearing its top. The histological findings, which showed epithelial necrosis characteristic of neurotic skin gangrene, are described in detail.

Experiment B. Subject: a nurse, not hysterical, easily hypnotized. Two years before she had suffered from neurotic skin gangrene.[1] Since that time, after warm baths or severe mental excitement there would appear on her skin red spots and wheals. In deep hypnosis she was touched with a match stick and told that a blister would form immediately. The place touched, the inner surface of the left upper arm, was covered with a stiff cardboard tube which went over the elbow and into the axilla. It was tied with bandages, which were signed with an ink-pencil. Inside of the cardboard tube there were put a definite number, known only to the experimenter, of porcelain balls, "which gave a definite color reaction by the drying out of a non-stimulating chemical substance." (The rationale of the control with the balls is by no means clear.)

[1] There is a difference of opinion as to the nature of neurotic skin gangrene. By some authorities it is considered of hysterical origin; i.e., the patient produces the injuries mechanically or by burning himself. In some American textbooks this is the only interpretation of the disorder given. Kreibich did not share this opinion. Cf. Dunbar (5, 382).

At 4 p.m. (it is not stated when the experiment began), the bandage was perfectly intact. Since it was too tight, it was removed. The balls were counted and all were recovered. On the skin there was a red spot of the size of a crown coin. A delicate pink color around this area indicated a more extensive hyperemia. In the middle of the red spot there was a yellowish-white bloodless area; its paleness resulted from edema of the papillary body. The same bandage was put on and removed next day, 24 hours after the suggestion. The area was excised. The histological findings were consistent with what the authors have described in connection with neurotic skin gangrene as dilatory erythema. Over the whole area the epidermis is connected with the cutis, at no place raised in the form of a blister, and in the epidermis there are the most varied degrees of epithelial necrosis. Further histological details are to be found in the original article.

1909. Heller and Schultz, of the dermatological clinic of the City Hospital, Frankfurt-am-Main (9). Subject: a carpenter, 19 years old, extraordinarily suggestible, who had been used as subject by several lay hypnotists; he could slow down his heart beat in the trance to about 30 beats a minute. Delicate skin, dermatographia of medium grade, but no urticaria factitia or other signs of a specially labile vasomotor system. At 11 a.m. a mark coin was laid on the back of the left hand, and he was told that a painless burning was occurring which would produce a blister by 5 p.m. When the coin was lifted, a red spot on the skin was noticed, corresponding exactly with the size of the coin. Professor Herxheimer, director of the clinic, bandaged the hand with a cover of cotton-wool, closed on all sides, and sealed the knots. The subject was now awakened and felt no pain. At 5 p.m. on removal of bandage, there was a round, red, and slightly raised efflorescence of the exact size of the coin. The left half was like a wheal and was of a brighter red, and in the lower left quadrant the epidermis was raised to form an irregularly shaped, flat, hard blister. The bandage was again put on. Thirty hours later, the blisters had flattened but the redness was still plain. After 5 days the blister had healed with a bright red scar. The authors say that they were justified in concluding that their case did not correspond to

urticaria factitia, in which scars never occur, but that it involved epithelial necrosis.

1909. Podiapolski, a physician of Saratov, Russia (**14**). Subject: a healthy peasant woman, a good somnambulist who five years previously had been relieved by hypnosis of hysterical loss of voice due to fright. At that time Podiapolski had produced a blister and had published an article about it. In 1908 he read of a discussion of the Société de Neurologie (Paris), in which Raymond affirmed and Babinski denied the possibility of producing blisters. This controversy renewed his interest, and he called a conference of 5 physicians to plan an experiment in which all precautions should be taken to guard against fraud or error. Subject was put into deep hypnosis in a hospital in the presence of 3 other physicians. At 1 p.m. 2 burns were suggested in a place on her back that would be hard to reach. Stimuli: a coin and the convex bottom of a metal thermometer case, applied with light pressure. Subject was not allowed to see them or to know what they were. Subject was then placed on a perfectly smooth couch without any projections and left there to sleep. Every possibility of intentional rubbing was removed, and a nurse was employed to watch her without interruption. In addition to the nurse, Podiapolski and 2 other physicians were in the room nearly all the time. At 4 p.m., subject was brought out of the trance. At 8 p.m., 2 erythemas were noticed, which were not exactly in the places stimulated. At 10:30 subject fell asleep until midnight, when she awoke and complained of pain and said that a blister on her back had opened up. This was found to have occurred. She had been sitting as she habitually sat, without leaning back, in accordance with the habit developed by Russian peasants, who usually had only backless benches in their houses. Subject could even fall asleep in a sitting position with no support but her left hand. She did not remain for a moment without being watched, and "those on watch duty literally did not remove their eyes from her." No attempt to injure herself was noticed. The stimuli were applied at the upper and lower corners of the shoulder blade. One of the blisters was about 3 cm. from the spot stimulated; the other was 1 cm. off. There was a round, red field, slightly swollen and corresponding to the size

of the coin, with 3 or 4 small blisters inside it. The effect of the application of the thermometer case was a single blister, which had burst.

1912. Smirnoff of Moscow (**19**). Subject: a strong and healthy peasant girl, 19 years old, whom Smirnoff had treated successfully with hypnosis for toothache. First experiment: A triangular collar button, which Smirnoff pretended to heat over a lamp, was laid on the rear surface of the upper arm. Subject was kept in the trance 15 minutes after the suggestion. Shortly before her awakening, a reddening of the skin was seen, elliptical in shape, with the long axis lying in the same direction as the long dimension of the button. The redness was somewhat lower than the place touched by the button. Fifteen minutes later, a white blister formed, around which the redness extended for 4 cm. Second experiment, 4 days later, in the presence of Podiapolski and another physician. Stimulus: a flat copper letter C (dimensions not given). Painful burning was suggested. Trance was prolonged 1 hour after stimulation. Towards the end of the hour redness, 4 cm. in diameter, was noticed, not at the place of stimulation but around the "burn" produced in the first experiment. This effect soon disappeared, and another erythema appeared between the old "burn" and the place just touched. The place occupied by the new redness became gradually whiter and more swollen and in the course of 2 hours after the suggestion a white blister formed, about 1 cm. long. Simulation is ruled out, since "the whole process went on before the eyes of the observers."

1915. Wetterstrand (**1**). The experiment was performed in the presence of Alrutz, a psychologist, and Sederholm, a dermatologist, in 1905. Subject: an hysterical woman, 55 years old. Suggestion given at 10 a.m. that burning sealing wax was being poured on a place on the volar side of the right forearm. There is no account as to what stimulus, if any, was applied. Subject complained of pain. A round glass plate held in metal and rubber rings, 4 cm. in diameter, was tied over the place. The glass was 7 mm. above the skin. The ends of the bandage that held the plate on the arm were sealed. At noon, the suggestion was given that a blister would form by 2 p.m. At that time "a possibly beginning reddening" was seen. The bandage was removed, and an adhesive plaster bandage with a dome-

shaped celluloid pane was put on. Around this a gauze bandage was put; its ends were sealed with sealing wax in 2 places and signed. It is not stated when subject was brought out of the trance. Next day, 11 a.m., subject returned and reported pain; the bandage was intact. Twenty-four hours later the fully intact bandage was removed. Blisters could be seen through the celluloid plate in the adhesive plaster, which also was intact. A protocol from the dermatologist is given, testifying briefly with respect to the bandages and the appearance of the blister.

1917. Hadfield (7). Subject: Leading Seaman P, being treated for "shell shock" in a naval hospital. First experiment. The suggestion was given in the trance that the anterior aspect of the forearm was being touched with a red-hot iron and that a blister would form. Subject was watched for 3 hours, part of the time in the waking state and part in the trance. The arm was then bandaged with a large roller bandage so that it would be impossible for subject to interfere; the bandage was pinned with a safety-pin and the pin sealed with sealing wax. The subject was then sent to the ward, suffering great pain. He returned in 6 hours from the beginning of the experiment. A blister had formed on the spot touched, a white patch of dead skin in the center with a small amount of fluid underneath and hyperemia around it.

Second experiment, with stricter precautions. The lateral aspect of the upper arm was used. "I was never personally left alone with the patient; the patient was never left alone; and I personally never touched the arm of the patient, this being done by another surgeon present, whilst I made the verbal suggestions. Throughout the day the patient was watched, and at night he was not only watched by the night nurse but his arm was securely bound up and sealed as before." Next morning, 24 hours later, the bandage was removed in the presence of 3 surgeons. The seal and bandage were intact. On the spot that had been touched there was "the beginning of a blister as before, which gradually developed during the day to form a large bleb with an area of inflammation around." The longer time required in the second experiment is attributed to the fact that for some hours the arm was exposed to cold air and also probably to the extra thickness of the skin on the upper arm.

In 1920 Hadfield (8) reported experiments on reducing the

temperature of the skin of a very susceptible subject, on whose skin he could also produce blisters. He gives no details on the blister-formation, as the case was similar to the previous one.

1927. Schindler (**16**). Subject: a woman, 38 years old, who had suffered for 6 years from extensive and painful ecchymoses, spontaneous in appearance and hysterical in origin. The trouble was diagnosed as hysterical after several years of ineffective somatic treatment. After a good many trials, it became possible to produce ecchymoses by hypnotic suggestion at spots indicated to the subject in the trance. In one experiment, an area on the forearm was marked off with a grease pencil. A thick plaster cast was then put on, extending from the middle of the upper arm and enclosing hand and fingers. After 2 days the cast was removed, and the ecchymosis was found, corresponding quite closely to marks. In one of these experiments, she was told that the place pointed out would "burn." The purpose of this suggestion was not to produce a blister but merely to insure the production of the ecchymosis. The ecchymosis appeared, but in its center there was a blister containing several drops of clear serum. On the next day there was a hemorrhage into the blister. The following entries are from the clinical record:

> Jan. 20, 1921. At present spontaneous blisters appear daily, without suggestion, on the most varied parts of the body, always on the top of an ecchymosis. They progress, in general, as did the first one. However, now and then the bleeding into the blister does not occur.
>
> Jan. 26, 1921. The production of a blister can now be controlled to the minute. It shoots up before the eyes of the doctor at the appointed time and is fully developed in 5 minutes. Such a blister is suggested, and a plaster cast with a watch glass is put on. The blister appears and is demonstrated in the medical society.
>
> Jan. 28, 1921. Today a blister appeared without previous ecchymosis.

The above entries constitute the whole of the regrettably short record relating to blistering. The patient was entirely relieved of her symptoms by hypnosis and change of environment, although the particular psychogenesis of the trouble could not be found out.

Some articles that are scientifically without value are not abstracted but are listed in the bibliography (**6**; **12**).

To summarize: Of the 11 subjects mentioned above, 3 were normal, 2 were normal but had suffered from hysterical aphonia and neurotic skin gangrene respectively, and 6 were hysterical.

Hadfield's second subject is not counted because of the absence of details concerning the control. The time required for the development of the skin reactions varies enormously—from 10 minutes to 24 hours for the blister, from a few seconds or minutes to 7 hours for the erythema. In 2 cases it is stated that the skin response did not occur exactly at the area stimulated. In the last 55 years there have been only 10 articles written in which investigators have reported the formation of blisters and given a reasonably full account of their procedure and control of the subject.

The writer, after all this evidence, still finds himself in an attitude of suspended judgment, an attitude due mostly to his inability to understand by what physiological processes suggestion—or the central nervous system—could produce *localized and circumscribed* erythemas or blisters. The results of actual thermal or mechanical stimulation of the skin are due (1) to an increase in the permeability of the walls of the capillaries and a consequent exudation of fluid and (2) to dilatation of the arterioles. The first effect is caused by a local liberation of histamine (or a similar substance) in the injured tissues. The second effect is due to axon reflexes. Both of these processes are independent of the central nervous system. In addition to these two processes, the skin may in certain circumstances be subject to the influence of antidromic impulses. These impulses, set going by irritation of the dorsal root ganglion of the spinal cord, produce the vesication observed in herpes zoster. In the laboratory antidromic action can be produced by artificial stimulation of the ganglion. There seems to be no evidence that antidromic impulses ever occur except in these two cases. Even if they were held responsible for hypnotic vesication, one would still have to explain how these impulses are produced by cerebral influence and how they could produce localized effects.

It may be objected that one should not deny the existence of a fact or what appears to be a fact merely because it cannot be explained in the light of present physiological knowledge. To this objection the answer may be made that the alleged fact needs stronger experimental evidence than the clinical observations given above afford. The observers have looked for blisters, which are the final result in a process involving several stages. If in certain rare cases blisters have been produced, we should

reasonably expect to find, in a larger number of cases, skin reactions that do not go so far as vesication but end in erythema. Further research should be carried out on a large number of subjects in order to see if such slight skin reactions to suggestion can be detected. In order to eliminate the effects of mechanical stimulation, a spot of light from which all heat rays have been filtered should be used as a stimulus in some of the experiments. In order to detect the slightest degree of erythema, photography with the proper type of film and filters should be used in order to detect vasomotor changes in the skin that are too slight to be visible to the eye. The surface temperature inside and outside of the "burned" area should be taken. Other precautions will suggest themselves to any one trained in experimental technique. Only further research will dispel the doubts concerning this fundamental problem of hypnosis.

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MEMORY LOSS IN SENESCENCE

BY JEANNE G. GILBERT
Brooklyn, N. Y.

MEMORY is popularly conceded to diminish with advancing years. Old people complain that they cannot learn or remember as well as they used to, and young people deplore the annoying forgetfulness of their elders.

Scientifically, there seem to be some differences of opinion. Hollingworth, reporting a loss on the type of task which requires the establishment and exercise of new associations, states that memory is one of the earliest abilities to decline (5). Meumann (7) finds retrogression of memory occurring at about 40. Miles (8) likewise reports decline of learning ability with age. Gruhle (3) states that among the intellectual capacities, the ability to learn new material is most seriously affected. Kubo, in working with individuals 70 to 100 years of age, reports that rote memory does not show a sudden fall until the age of 82, when it decreases rapidly (6). The writer similarly finds a marked loss in the type of memory involved in the formation of new associations, but relatively little in immediate rote memory (2).

On the other hand, Sorenson (9) concludes "that at least up to fifty there is no decline in learning ability with age for those actively engaged in study." Thorndike, although he finds a gradual drop in learning ability after the maximum at 20–24 years, reaches a similar conclusion (11). Hamilton (4) suggests that the lack of educability in the aged and their impairment of memory for recent events may be psychogenic in origin and correctable.

More recently, Wechsler (12) states that although memory span declines rapidly with age, it nevertheless holds up better than certain other abilities and he concludes, therefore, that "older people have other failings of which they might much more reasonably complain than their poor memory."

Perhaps in speaking of memory loss, it would be wise to clarify matters by being precise in terminology. Can one, from results on two memory-span tests (Digits Forwards and Digits

Backwards), conclude that memory either does or does not decline with advancing age? It is well recognized that simple memory span and logical or associative memory are two quite different abilities, by no means perfectly correlated; Wechsler himself points out the limited scoring range for repetition of digits and the fact that the usefulness of the test is limited largely to a necessary minimum. What, then, is the justification for generalizing from this to memory decline?

In order to gain an understanding of the real nature of memory loss in older persons, we must differentiate between the various types of memory. We must study not only memory span but the ability to form new associations necessary to the learning of meaningful material or the reproduction of material beyond simple memory span. We must study both verbal and non-verbal memory, immediate, free recall of meaningful verbal material, immediate, controlled recall, and retention of both types of material. Only thus can we gain insight into the nature of the learning process of older persons and determine wherein the memory difficulty occurs.

This present study presents the results of eleven different types of memory tests: three digit-memory-span tests (visual, auditory and reverse), repetition of sentences, Knox cubes, immediate reproduction of a paragraph, delayed recall of the same paragraph, drawing of designs from memory, paired associates, retention of paired associates, and retention of a Turkish-English vocabulary. No tests involving speed of recall or of motor ability were included in the series. The designs, Knox cubes, and Turkish-English vocabulary may be said to involve some degree of motor ability, particularly because the use of a pencil is required for the drawing of the designs and the underlining of the words in the Turkish-English vocabulary; but the degree of motor involvement is slight and speed is no consideration.

To make a truly accurate study of memory loss, it would be necessary to examine each individual at regular intervals throughout his life in order to find first his peak of memory functioning and then to determine how much he had declined from this peak at any given period of his life. As such a procedure is obviously impossible when making a current study of senescents, the alternative is to compare the memories of com-

parable groups of senescents and of young persons at the peak of their efficient functioning. The intellectual levels of the two groups must be matched, so that the discrepancy between the memory scores of the two groups will be indicative of the decline of memory of the older group. While only a longitudinal scale could give us the precision we desire in measuring loss of memory, the impracticality of the longitudinal method forces us to the use of this method which tells us, by inference, wherein the decline occurs.

In this study Terman's vocabulary (1916 revision) was used as the control of intellectual level (**10**). As the use of vocabulary for control of original intellectual level has been fully discussed elsewhere and shown to be especially valuable for use with older persons (**2**), it needs no further discussion here.

One hundred seventy-four individuals between the ages of 60 and 69 years inclusive were matched exactly for vocabulary level with 174 individuals between the ages of 20 and 29 years inclusive (average Terman vocabulary 16.62 years for both groups). Members of each group were given the same tests of memory, the discrepancy between the scores of the two age groups serving as the measure of loss on each test.

Each individual in this study was tested separately for vocabulary level and for the various types of memory.

The three digit-memory-span tests—visual, auditory and reverse digit spans—were presented in the usual way, the highest number repeated by the individual being his score in that test.

The sentence repetitions consisted of the series given in the Babcock Test of Mental Efficiency (**1**). Individuals were scored 1 for each sentence repeated without error and one-half for each sentence repeated with only one error, final score being the total number correct.

Score on the Knox cube test was the number repeated correctly.

The test used for the memory of paragraphs was that found at the 10-year level of the Stanford-Binet. Score was the number of memories produced correctly.

Retention of this same paragraph was tested later, the score likewise being the number of memories produced correctly.

Designs from memory were given as in the Babcock Test of Mental Efficiency and points credited similarly (**1**).

The two lists of 9 paired associates given in the Revised Babcock Test of Mental Efficiency were presented alternately to members of each group, the average number right in two trials being the final score. The paired associates, however, were not weighted for time as in the Babcock test.

Retention of the paired associates was tested later, score being the number given correctly.

Early in the interview, the individuals were presented with the Turkish-English vocabulary as given in the Revised Babcock Test. After completion of it, they were given thirty seconds in which to study the vocabulary. At the end of the tests they were presented with only the English words followed by three Turkish words (the English words with their Turkish equivalents being omitted). Score was the number of Turkish words correctly underlined.

RESULTS

A. *Comparison of scores on the separate tests.* Table 1 shows the average scores of the two groups on each of the memory tests, the variability of the scores, the reliability of the averages, the differences between the scores of the two groups, the reliability of these differences and the percentage of the sixty-year-old group reaching or exceeding the mean of the twenty-year-old group.

It will be noted that the differences between the average scores of the two groups on all tests are completely reliable, although in some cases the differences are relatively small.

The differences on visual and auditory digit spans are small and the amount of overlapping large. Deterioration is a little more obvious in the reversed digit span, as more than a mere ability to receive new impressions is involved. The digits must be held in mind for a longer time than when repeating them in the order given, so that more concentrated effort and mental control are involved.

Sentence repetitions, while a test of immediate memory, are different from digit repetitions in that meaning is involved. While a digit span tests primarily the ability to grasp a number of discrete units in one moment of attention, a sentence span involves not only this ability but also a comprehension of meaning. These two factors complicate the task, and in some cases meaning may be neglected in an effort to reproduce the exact

words, with the result that a certain number of words in the sentence are given accurately and the rest entirely lost. In other cases, meaning is grasped at the expense of accuracy and the idea but not the actual wording of the sentence is given.

TABLE 1

A COMPARISON OF THE PERFORMANCES OF THE TWO GROUPS ON VARIOUS TESTS OF MEMORY

TESTS	AVERAGE SCORES	S.D.	$\sigma_{aver.}$	DIFF.	$\sigma_{diff.}$	$D/\sigma_{diff.}$	PERCENTAGE OVERLAPPING
Vis. Dig.							
20 yr. gr.	8.21	1.11	.0841				
60 yr. gr.	7.51	1.45	.1009	.70	.138	5.07	42
Aud. Dig.							
20 yr. gr.	6.87	1.15	.0871				
60 yr. gr.	6.06	1.15	.0871	.81	.123	6.59	33.8
Rev. Dig.							
20 yr. gr.	5.53	1.35	.1022				
60 yr. gr.	4.36	1.22	.0924	1.17	.138	8.48	26.5
Sent. Rep.							
20 yr. gr.	16.76	2.72	.2058				
60 yr. gr.	13.29	2.09	.1583	3.47	.259	13.40	27
Cubes							
20 yr. gr.	8.71	1.95	.1629				
60 yr. gr.	6.44	2.15	.1485	2.27	.221	10.27	20
Paragraph							
20 yr. gr.	10.85	3.22	.2241				
60 yr. gr.	6.32	3.33	.2517	4.53	.351	12.91	12
Ret. of Par.							
20 yr. gr.	14.01	3.47	.2642				
60 yr. gr.	9.16	4.47	.3388	4.85	.430	11.28	17
Designs							
20 yr. gr.	13.54	2.65	.2009				
60 yr. gr.	7.32	4.48	.3394	6.22	.394	15.78	13.9
Paired Assoc.							
20 yr. gr.	6.27	1.91	.1448				
60 yr. gr.	2.59	2.09	.1501	3.68	.209	17.61	10
Ret. of P. Assoc.							
20 yr. gr.	7.51	1.75	.1326				
60 yr. gr.	3.41	2.38	.1840	4.10	.224	18.63	10.9
Ret. Turk.-Eng. Voc.							
20 yr. gr.	7.49	1.73	.1311				
60 yr. gr.	2.97	2.58	.1956	4.52	.236	19.15	12

Although a simple memory span which involves neither meaning nor language, the Knox cubes require greater concentration than either digit or sentence repetitions. Possibly also, both a kinæsthetic imagery and a motor factor are involved. It will be noted that the difference between the average scores

of the two groups is relatively larger in this test than in the other memory-span tests and the amount of overlapping is smaller. As on most of the tests in the series, the variability of the older group is greater than that of the younger group.

The immediate memory of a paragraph involves meaning and memory which is definitely beyond that of simple repetitions, whether these repetitions involve meaning or not, and here a more serious loss can be observed.

Retention or delayed recall of the above-mentioned paragraph requires that the individual retain certain facts in his mind for a period of time during which he is engaged in other mental activity. It involves not only initial concentration and the grasping of meaning but also the formation of more or less lasting associations which can be recalled at will. Again, a marked difference between the two groups will be noted.

The memory for designs is a non-verbal test of memory and may be said to involve some degree of motor skill, as the use of a pencil requires a certain amount of motor skill. The involvement of motor skill, however, is too slight to be considered a serious handicap to the older group. Memory, imagery, and perception of spatial relations are far more important factors, especially as speed of execution does not enter into the picture. The difference in scores is striking and the greater variability of the older group is again noticeable.

Marked difference between the two groups is found in the learning of paired associates. This test requires the formation of entirely new and unfamiliar associations. The pairs of words have no logical connection and the individual must find in his own mind some association which will bind the pairs together. This task is especially difficult for older persons.

Retention of paired associates is one of the tests on which older persons show the greatest decline. We see that not only is the formation of entirely new associations harder for older than for younger persons but that the retention of these associations is even more difficult.

The older group also shows distinctive loss on the retention of a Turkish-English vocabulary test. While this might appear to be entirely a test of verbal memory, it is probable that, in many cases at least, the verbal factor is secondary to other non-verbal factors. Instead of remembering the actual Turkish

words, many individuals remember the positions of the words they underlined when first presented with the series. A kinæsthetic memory seems to play a part.

B. *Percentage of loss of the older group on the tests.* In order to show more clearly the relationship between the various types of memory decline, the per cent loss of the older group on each test is presented:

PER CENT LOSS	TEST
8.5	Visual Memory Span for Digits
11.8	Auditory Memory Span for Digits
21.2	Reversed Digit Span
21.3	Sentence Repetitions
26.2	Knox Cubes
39.7	Retention of Paragraph
41.8	Immediate Memory of Paragraph
45.9	Memory for Designs
54.6	Retention of Paired Associates
58.7	Paired Associates
60.4	Retention of Turkish-English Vocabulary

Examining these percentages, we see marked differences on the various types of memory tests. For example, if we combine those tests involving immediate repetitions (tests 1 through 5), we find an average drop of 17.8 per cent. Contrasting this with the loss on those tests which can be considered non-repetitive in character, we find an average recession on the latter tests (tests 6 through 11) of 50.2 per cent, a difference of 32.4 per cent.

Comparing immediate verbal repetitions with meaningful verbal memory, we find an average difference of 13.2 per cent for the verbal repetitions and a loss of 51 per cent for the meaningful verbal memory.

Likewise with non-verbal immediate repetitions (Knox cubes) and non-verbal memory (memory for designs) which is non-repetitive in character, we note recession of 26.2 per cent and 45.9 per cent respectively, a difference of 19.7 per cent.

Of the repetition tests, we find that those involving meaning or greater concentration show the greatest loss. For example, the visual and auditory digits, which involve no meaning and require less concentration than the other tests of immediate repetitive ability, show an average reduction of 10.5 per cent as compared with an average loss of 22.9 per cent for the reversed digits, sentence repetitions and Knox cubes, a difference of 12.8 per cent.

We find greater loss on the verbal learning tests when the recall is controlled (paired associates, retention of paired asso-

ciates and retention of Turkish-English vocabulary) than when uncontrolled (immediate recall of paragraph and retention of paragraph), whether the recall be immediate or delayed—a difference of 16.9 per cent for immediate recall and 17.8 per cent for retention. This is probably because the necessary associations to be formed in the controlled learning tests are entirely new and may even interfere with previously formed associations.

There is slightly greater decline in retention of verbal material, whether the learning be controlled or uncontrolled, than in immediate memory but the difference is slight. Average loss for retention is 52.4 per cent; average loss for immediate memory is 50.3 per cent; difference 2.1 per cent.

C. *Memory loss in the brightest older persons.* In the course of the tests, it seemed that the very brightest individuals in the older group were showing less memory failure than those not quite so bright. Of course, bright older people would be expected to have better memories than their less intellectual fellows, but it seemed that these individuals were also showing *relatively* less loss; that is, they were not showing as much drop from their own original level as others in the sixty-year-old group.

In order to measure this seeming difference, the 40 individuals in each group having the highest intellectual level—that is, those knowing 80 or more of the 100 words on the Terman vocabulary list—were separated from the others and their scores compared. The bright sixty-year-old group was also compared with the total twenty-year-old group. Table 2 compares these groups on each of the tests.

It will be observed immediately that on every test the bright sixty-year-old group shows less loss than the sixty-year-old group considered as a whole; not only, as might be expected, when compared with the total twenty-year-old group but also when compared with a twenty-year-old group matched for intellectual level.

The bright sixty-year-olds show the same type of memory decline as the total group of sixty-year-olds, but their loss is both relatively and absolutely less than that of the total group. They show the least recession when the test is one of memory span not involving meaning, and the greatest differentiation when the test involves the formation and retention of entirely new associations. The differences in scores between the bright

TABLE 2

A COMPARISON OF THE BRIGHT SIXTY-YEAR-OLD GROUP WITH THE BRIGHT TWENTY-YEAR-OLD GROUP AND WITH THE TOTAL TWENTY-YEAR-OLD GROUP

	AVERAGE SCORES	S.D.	$\sigma_{aver.}$	DIFF.	$\sigma_{diff.}$	$D/\sigma_{diff.}$	TOTAL 20-YEAR GROUP		
							DIFF.	σ_{diff}	$D/\sigma_{diff.}$
Vis. Dig.									
Br. 20's	9.28	.89	.1248						
Br. 60's	8.80	1.12	.1769	.48	.217	2.21	+.59	.198	2.98
Aud. Dig.									
Br. 20's	7.28	1.28	.1991						
Br. 60's	6.78	1.15	.1816	.50	.235	2.12	—.09	.197	.46
Ker. Dig.									
Br. 20's	6.40	1.45	.2291						
Br. 60's	5.20	1.06	.1674	1.20	.283	4.24	—.33	.204	1.62
Sent.									
Br. 20's	19.35	1.80	.2843						
Br. 60's	17.35	1.94	.3065	2.00	.418	4.78	+.59	.370	1.59
Cubes									
Br. 20's	9.38	1.59	.2511						
Br. 60's	8.03	2.17	.3428	1.35	.425	3.18	—.68	.364	1.87

TABLE 2—*Continued*

	AVERAGE SCORES	S.D.	$\sigma_{aver.}$	DIFF.	$\sigma_{diff.}$	$D/\sigma_{diff.}$	TOTAL 20-YEAR GROUP		
							DIFF.	$\sigma_{diff.}$	$D/\sigma_{diff.}$
Parag.									
Br. 20's	12.25	3.16	.4992						
Br. 60's	9.73	3.29	.5199	2.52	.721	3.50	—1.12	.574	1.95
Ret. of Par.									
Br. 20's	16.50	2.47	.3902						
Br. 60's	13.43	3.23	.5102	3.07	.642	4.63	—1.58	.978	1.62
Designs									
Br. 20's	15.20	1.30	.2053						
Br. 60's	11.48	3.78	.5972	3.72	.631	5.89	—2.06	.729	3.28
P. Ass.									
Br. 20's	7.38	1.28	.2021						
Br. 60's	4.70	2.24	.3538	2.62	.408	6.57	—1.57	.383	4.09
Ret. of P. Ass.									
Br. 20's	8.25	1.23	.1943						
Br. 60's	5.75	2.24	.3539	2.50	.404	6.19	—1.76	.378	4.66
Ret. T.-E.									
Br. 20's	8.53	1.14	.1801						
Br. 60's	5.80	1.36	.2147	2.73	.281	9.72	—1.69	.252	6.71

twenty-year-olds and the bright sixty-year-olds, although smaller than the divergences between the total groups, are completely reliable except in the visual and auditory digit spans, when the chances are 98 to 99 in 100 that the true differences are greater than zero.

In comparing the bright sixty-year-olds with the total twenty-year-old group, we find the older group superior to the younger group on visual digit span and on sentence repetitions, the difference in digit span being an almost completely reliable difference. Of the 9 differences in favor of the younger group, only 4—again, those requiring the formation and retention of entirely new associations and also non-verbal memory—show complete reliability.

The results obtained when comparing bright sixty-year-olds with bright, average, and dull twenty-year-olds should serve to those working on decline of abilities with advancing age as a caution that original intellectual level must be adequately controlled. Only when we are certain that we have intellectually comparable groups of young and old can we compare the functioning of young and old, whether the functioning be in the nature of memory, motor ability, speed, or any other ability. Decline in functioning is significant only when we know from what level the functioning has declined; this diminution can be ascertained when we know the original intellectual level and the normal peak of functioning for that level which, in most abilities, is reached in the twenties.

D. *Percentage of loss of the bright sixty-year-old group.* The differences in the percentage of loss suffered by the bright sixty-year-old group show more clearly the less memory loss which occurs in the brighter group. The following shows the percentage of decline on each test when the two bright groups only are considered:

PER CENT LOSS	TEST
5.2	Visual Digit Span
8.4	Auditory Digit Span
10.3	Sentence Repetitions
14.4	Knox Cubes
18.6	Retention of Paragraph
18.8	Reversed Digit Span
20.6	Immediate Memory for Paragraph
24.6	Memory for Designs
30.3	Retention of Paired Associates
32.0	Retention of Turkish-English Vocabulary
36.3	Paired Associates

It will be noted that while the order of per cent decline on the tests is slightly different with this group, the loss on types of tests remains the same. The change of order is within the groups of similar types of memory tests as, for example, paired associates and retention of Turkish-English vocabulary, rather than from one type to another. Repetitions still show the least loss; paired associates and retention of Turkish-English vocabulary the greatest. However, the greatest recession of 36.3 per cent for this group as against the maximum decline of 60.4 per cent for the total group—a difference of 24.1 per cent—is significant. Other figures are also significant: an average recession on all the memory tests of 20 per cent for the bright older group as against 35.5 per cent for the total older group; an average decline on all immediate repetitions of 11.4 per cent as against 17.8 per cent; and an average loss of 27.7 per cent on all non-repetitive tests as against 50.2 per cent for the bright and total older groups respectively.

SUMMARY AND CONCLUSIONS

This has been a study of memory loss at the beginning of senescence. In order to insure measurement of actual memory decline, original intellectual level was controlled by matching the 174 senescents exactly for vocabulary level with 174 individuals in the twenties and giving each individual in both groups 11 tests of various types of memory.

Results show completely reliable differences between the scores of the younger and older groups, giving clear evidence of an actual and reliable decrease in the functioning of the memory process at senescence.

There is unevenness of decline on the different types of memory, varying from a loss of 8.5 per cent on visual memory span for digits to a failure of 60.4 per cent on retention of a Turkish-English vocabulary.

There is least recession on visual and auditory digit-memory spans, tests of simple repetitions where meaning is not involved.

There is greater, but still moderate, decline on tests of simple repetitions where meaning or greater concentration is involved, such as reverse digit span, repetition of sentences, and Knox cubes.

When meaning is involved in uncontrolled, verbal reproduction of selections beyond the length of simple memory span, severe loss is found, whether the recall be immediate or delayed.

Non-verbal memory, as in drawing of designs from memory, shows great loss, namely 45.9 per cent.

The greatest obstacle is in the formation of entirely new associations, particularly if these may interfere in any way with previously formed associations, as in paired associates (both immediate recall and retention) and in the retention of a Turkish-English vocabulary.

Greater difficulty is always shown in non-repetitive memory than in immediate repetitions, whether the test be verbal or non-verbal.

On learning tests, controlled recall shows greater loss than uncontrolled, a circumstance which holds true whether the recall be immediate or delayed.

There is slightly greater loss for retention than for immediate recall, whether the recall be controlled or uncontrolled.

The brightest individuals show both relatively and absolutely less decline of memory of all types. When compared with comparably bright young people, they show an average loss of only 20 per cent on all memory tests as against an average recession of 35.5 per cent for the total older group.

When compared with the total younger group, the bright older group shows completely reliable losses on only 4 tests—designs, paired associates, retention of paired associates and retention of Turkish-English vocabulary. On visual digits and sentence repetitions, they are slightly superior to the total young group. These points emphasize the necessity of controlling original intellectual level when measuring decline of memory or any other ability with advancing age.

To conclude, older persons on the average show relatively little decline in ability to receive new impressions but suffer greatly in their ability to form the new associations necessary to the learning of entirely new material. Their learning process seems to suffer from their decreased flexibility and adaptability. They can and do learn new material, but the learning is laborious and the results less satisfactory than when they were young and at the peak of their efficient functioning. The brighter

they are to begin with, the better they tend to retain the efficiency of their learning process; while the real cause can only be conjectured, it may bear some relation to the greater flexibility of the bright persons and their continued activity in the intellectual field. Such continued activity and production should be fostered rather than wasted by enforced age retirement rules.

For older persons themselves who wish to compensate as much as possible for the normal memory decline accompanying advancing age, we might suggest greater practice on things to be remembered, methodical habits of jotting down things they wish to remember, making memoranda of new material they wish to learn or remember and then studying it at their leisure, remaining independent of younger people in so far as possible (i.e., avoiding the habit of depending on younger people to remember the things they should remember themselves), keeping active mentally (i.e., keeping alert to surroundings and interested in new activities), and, finally, maintaining the intellectual quality of their reading. This last applies particularly to those individuals of good intellect who in later years tend either to cease reading entirely or to read trashy literature they would have scorned in their younger days.

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SHORT ARTICLES AND NOTES

ANOTHER PSYCHOLOGIST ANALYZED

BY AUSTIN B. WOOD
Brooklyn College

Factors Leading Up to the Undertaking

I UNDERTOOK an analysis because I felt I needed it. In spite of my orthodox training in psychology, I had come to the point where I felt in need of help and where I guessed that psychoanalysis might offer the help I sought. An unhappy marriage had terminated in a divorce after five strenuous years, and my failure to make a successful or even tolerable marital adjustment had left me with a deep feeling of incapacity and inferiority in the sphere of intimate emotional relationships.

The self-searching that took place at this time brought me to the realization that I was a much less well-adjusted and effective individual than I had previously thought myself to be. I felt, vaguely, that I was capable of doing a number of things, in the spheres of social-personal relations and of professional work, which for one reason or another I never got around to doing. The question facing me, then, was, "Why don't I do these things which others in my position seem able to do?" Had I been more of a psychologist at that stage of the game, I might have posed the question differently and asked, "Why don't I want to do these things?" In any event, I was for the first time seriously questioning my own personality and wondering if something could be done about it. At this point one who had become very close to me asked if I had ever considered being analyzed. I answered that I had not and that, furthermore, as a trained psychologist, I knew the method was no good. (I had, as a matter of fact, never read a word by Freud and only a few sketchy accounts of psychoanalysis by non-Freudians.) The question, however, remained with me, and for the first time I really asked myself if psychoanalysis might be of some value in my case.

Shortly thereafter I spent an evening with a number of fellow psychologists. During the course of the discussion one of them, whom I knew and respected as a person of sound sense and high scientific standards, mentioned the fact that he had been analyzed. Surprised to learn of this circumstance, I made haste to see him again and question him about the process. When my own need and his tempered, yet favorable, report of his experience led me to investigate the matter seriously, I soon determined to undertake an analysis. After consulting two analysts, neither of whom impressed me favorably, I met one who seemed to be a person for whom I could feel a sense of respect and a certain warmth. She was an M.D. and a practising psychiatrist who had been analyzed by a Freudian.

I came to the undertaking, then, as an orthodoxly trained, academic psychologist, but I came not from professional motives. I felt that I was a "mess," that I had failed in marriage, and that I very much wanted help in straightening myself out and freeing those capacities that I vaguely believed or hoped

were latent in me. Though I was not sure that psychoanalysis would do all these things for me (my orthodox training had been too strong), I very much wanted it to; and I began the process, as nearly as I can recall, in the state of mind that here was a method able to do fine things for some others and perhaps able to do likewise for me. Adopting the hypothesis that analysis might help me, I determined to let the events to be observed during the process prove or disprove the hypothesis.

The analyst was a woman, several years my senior, who seemed to be both a wiser and a better psychologist than I. I also felt in her a person of warm sympathy, understanding, and tolerance, and soon learned that, no matter what I did or said, she would not reproach me. I therefore had little difficulty with the problem of transference, which occurred soon and to a considerable degree. Also, I seemed to have little difficulty with resistances involving the idea that I knew more than the analyst on some particular point—though of course plenty of resistances in other forms developed.

THE PROCESS

In reviewing my experiences as an analysand, I find that they appear to have involved primarily two functions. First was the exploration of thoughts, feelings, memories, phantasies, and dreams; and second came the reorganization of patterns of feeling and behavior.

Exploration. The main problem areas explored were: (1) immediate problems—emotional relations with present intimates and conflicts over problems connected with my work; (2) relationships, present and past, with my mother—involving love, dependence, antagonism, resentment; (3) relationships with my sibs—involving jealousy and rivalry, both conscious and unconscious, and also love and pride; (4) relationships with my father—involving fear and hate, dependence, authority, jealousy, withholding of approval (love), criticism, and love; and finally the relationships of all of these prototypes to feelings and behavior involving other people and situations.

This exploratory process was carried out mostly by the recall of memories or phantasies (and often it was impossible to tell which was which), and of dreams, and a form of "free association" which took some element of memory, phantasy, or dream as its starting point. The questions most frequently put were: "Does that remind you of anything?" and "And how did you feel about that?"

The answers to these questions seemed to serve several purposes. For one thing, they enabled the analyst to gather information concerning my personality and to formulate hypotheses which she would occasionally offer, for my acceptance or rejection, concerning my ways of meeting various life situations. They also enabled her to summon me back to the problem at hand when, as so frequently happened, resistance developed and that part of me not under conscious control began to operate in defense of my ego, causing my associations to veer away from some painful topic. But more important than either of these was the fact that I was encouraged to put each of the questions to myself, and to answer them, to myself, as fully and frankly as I was capable of doing at the time. After awhile, in fact, I learned to anticipate the questions in many cases and to carry on the process of association with less prompting from the analyst.

Some of the early memories (or phantasies) came up so many times that

they were given identifying names. Others occurred but once. Many of them raised questions which I had never put to myself. Others brought certain facts or possibilities into a relationship never before consciously experienced. As a result, most of them, when they had been recalled and I had answered questions concerning the feelings involved and so forth, contributed to a growing understanding of my own "behavioral world" and the factors, internal and external, determining it.

Reorganization.

1. Role of understanding and of conditioning. Following the first steps of exploration, but generally paralleling the later steps, was the process of reorganization of feelings and behavior patterns which resulted, often automatically, from the broadened and deepened understanding gradually being acquired. Yet this improved understanding was far from being the sole or even the most important therapeutic aspect of the process. Rather, a form of emotional reconditioning was the most impressive of these effects. The results of this latter process were far more striking and reached deeper and wider than those which may be attributed to improved understanding. An example may make this clear.

My father was a very undemonstrative New Englander. His regime in the home was strict, he was always right, and he was unable as a rule to show more than the mildest manifestations of love or affection. And it was he, rather than my mother who was usually the one who said "No." My mother, on the other hand, was more or less the opposite. All these and other things produced in me a picture (conscious) of Mother as all good and Father as all bad, as far as I was concerned. One aspect of our family life was an almost continuous conflict between Father and myself, punctuated by hot and bitter arguments in which we both got very angry. Analytical explorations had turned up many recollections of events which I now saw, not from the point of view of the child that I had been when the event occurred, and with a child's fears and uncertainties, but from that of the young adult that I was at the time of analysis. I had seen, moreover, that many of these things pointed to the two-sided proposition that my father was not entirely the villain of the piece but had loved me and I him. At this point I began to recall a rather striking event which had transpired when I was eleven. I was run over by a street car and one hand was badly crushed. My father was called, came immediately, took me in his arms, and carried me the two blocks from the scene of the accident to our home. As he was carrying me home, I was crying loudly. He told me, "Be a big man, and stop crying," and I stopped.

Now this series of events was such a dramatic and intense experience that I might have been expected to remember it very vividly, and so I did. I had probably recounted it, just about as given above, on the average of once a year since it occurred; thus it belonged to that class of memories available to consciousness. But, in the analysis, when I reached the point in recalling the experience where I stopped crying in response to my father's "Be a big man, and stop crying," the analyst asked, "Why did you stop?"

This was a question I had never asked myself. All previous accounts I had given had gone right on from there to recount subsequent events. So I lay there and recalled the experience as vividly as possible and tried to see what there was in the whole situation that made me stop crying. What came was an "aha" experience, but emotional rather than intellectual. The situation was

that of my father, holding me tenderly in his arms, and very much concerned over me. The barriers to showing his love for me were down. And the very tone of voice in which he spoke said far more than his words. It said, "I love you very dearly, and I know that you are a big man [and this too was very important to me], and that you will be man enough to stop crying if I ask you to." These two experiences—his deep love for me, and his confidence in my manliness—struck with such force that I lay for several minutes weeping. When the emotion had passed, I felt that I had had a deep and intimate experience of the warmth and devotion between my father and myself, an experience which has affected my relations with him (and with other people and institutions for which he had become the prototype) ever since. It seems to me that the effect of this emotional experience on my subsequent personality was not less for its having been delayed some twenty-five years.

This process, the "catharsis" of Freud, occurred a number of times and always in the same general form. A complex was present. That is, an idea or event, and its attendant emotions, had been repressed because of some fear, of real or imagined danger. (In this case it was fear of the consequence of expressing, or even feeling, love for that forbidding person, my father.) When such fears have been brought to light and the patient reassured concerning them, the original experiences can be relived fully, without suppression of their emotional content, and reorganization of the sort described may follow.

And so it was for other areas of reorganization. Much was accomplished by increased or changed understanding and by the acquisition of new meanings or values; but beyond this, important reorganization of patterns of emotional behavior resulted from a number of such conditioning experiences.

2. Effects of Reorganization. Whatever the processes involved, the reorganization accomplished certain changes in the personality. Some of the feelings and desires which had been suppressed were liberated and given free expression. Others which had been weak were strengthened and given new functional meanings. Still others were redirected and given acceptable (to me and to society) channels or forms of realization.

3. Integration. Paralleling the reorganization of specific areas of personality or behavior, especially towards the latter part of the process, was the gradual articulation of these habits into a more or less integrated whole—the "post-analytic personality." When this had been accomplished, the analysis was terminated.

THE RESULTS

Personal. Without, I hope, undue complacency, I am far better satisfied with my "post-analytic personality" than I ever was with my "pre-analytic" one. Of course, life still has its problems, and I can even conceive that circumstances might possibly develop that would force me into a situation where I would need some further reorganization. But I think I know now what my capacities are, what I want from life, and how I can go about achieving my goals with the equipment I now have. And the prospect pleases.

Professional. The understanding of human motivation and behavior which the experience has brought me seems, as nearly as I can judge, to be at least as much as I had acquired in seven years of formal study and an equal number of years of teaching. This statement may overemphasize the significance of the insights into one's own motivations and those of others which the experience of analysis brings one. I, for one, do not think so.

THE DIES COMMITTEE AND PUBLIC OPINION IN 1939

BY STEUART HENDERSON BRITT
The George Washington University
AND
SELDEN C. MENEFEE
Washington, D. C.

IN 1938 the writers studied the effect on a group of college students of various newspaper reports of the Dies Committee's investigations (2; 3). The experiment demonstrated that use of the label "Communism" is quite effective in changing attitudes. A distinct shift toward disapproval of several names was found, notably The American League for Peace and Democracy, The American Civil Liberties Union, Harry Bridges, and the Communist Party, as well as Representative Martin Dies himself. There was little change, however, in opinion on some well-known political figures and labor organizations which were treated in the newspaper stories, such as President Roosevelt, Frank Murphy, the C.I.O., and John L. Lewis.

That experiment, in company with others of its type, is subject to two serious criticisms. First, the method was somewhat unrealistic, in that the propaganda material was read to the experimental group all at one time, with a questionnaire administered immediately afterward to determine what effects were produced. Actual newspaper articles were used, and the experiment was carefully controlled by the use of a before-and-after questionnaire in both experimental and control groups. But the situation outside the classroom is somewhat different from this set-up. People read newspaper stories on public issues over a considerable period of time, scanning many unimportant details as well as crucial reports of the kind selected for a classroom experiment.

A second objection is that both of the questionnaires were based on material which had already appeared in the newspapers, so that an unknown part of the effect which it was desired to measure had already been produced when the experiment started.

METHOD

With a view to reproducing in an experimental situation the actual spacing of newspaper stories on the Dies Committee, a new experiment was formulated by the writers early in 1939. The method was a radical departure from the one normally used in "shift-in-opinion" experiments, for it involved prognostication of two sorts. First, it was necessary to predict approximately when the Dies Committee would be holding hearings. Second, it was essential to guess in advance the general trend of the hearings, even to naming some of the persons and groups to be involved in the testimony.

The first step was to establish the timing of the experiment. Since the Dies Committee was not functioning during the early months of 1939, it was decided to give the questionnaire to a control group (Group I, 67 students) at that time, in order to determine what changes in response could be expected over a given period due to chance. The length of the period was set at six weeks on the assumption that the Dies Committee would be in session during about six weeks of the fall semester at The George Washington University. The questionnaire

was then to be given to two experimental groups during the period when the Committee was holding hearings. One of these groups (Group II, 46 students) was to be left alone during the experimental period, merely absorbing what they happened casually to read in their own newspapers about the Dies hearings. In the other group (Group III, 50 students), however, all newspaper stories on the Dies Committee which appeared in the *Washington Post* were to be read aloud at each session of the class during a period not to exceed six weeks. The questionnaire was to be given before and after the six-week period to both of these groups and to the control group.

The second step—composition of the questionnaire *prior to* actual hearings by the Dies Committee—presented a serious problem, since the success of the experiment depended largely on the correct prediction of items to be covered by the Committee some months later. On the basis of a considerable study of the past history of the Dies Committee, however, a list of 95 items was made up, including persons and organizations of radical, liberal, and Fascist leanings. The assumption was that the Committee would hit upon at least some of these in its hearings. In order to measure intensity of approval or disapproval, a five-point scale was used on each item, as follows: -2 = Disapprove; -1 = Probably disapprove; 0 = Doubtful; $+1$ = Probably approve; $+2$ = Approve. Opinions were completely anonymous, since only initials or some unique symbol of identification were used on the questionnaires.

On October 5, 1939, the questionnaire was filled out by the two experimental groups. During an ensuing period of five weeks, until the Dies Committee temporarily adjourned its sessions, it may be assumed that some members of Group II had been reading in their own newspapers certain of the accounts of testimony presented before the Committee. Group III met three times weekly during this same period, and at each session of the class the experimenter read to Group III without comment the current news stories about the Committee's hearings. When the Committee temporarily ceased meeting, the questionnaire was given again to both Groups II and III, a five-week period having elapsed for both.

RESULTS

Upon checking the newspaper clippings read to Group III against the questionnaire, it was found that almost 40 per cent of the items which were included in the questionnaire—37 out of the 95—had at least been mentioned in news stories duing the five-week period. Fifteen of these had been prominently or repeatedly mentioned, including three members of the Committee itself, as well as twelve individuals and organizations attacked in the newspaper reports of the hearings as subversive or as in some way associated with "Communist" activities. It was possible, therefore, to treat each of these fifteen names as experimental items. Thus, the before-and-after reactions to the fifteen items were tabulated for each of the three groups: Group I, which had no opportunity to read about any current hearings by the Dies Committee, since the Committee was not in session when the questionnaires were given; Group II, which could read any newspaper accounts of the hearings that they happened to see during the experimental period; and Group III, which heard at every class session the newspaper stories on the hearings which appeared in the *Washington Post*. The reliability of the responses to the fifteen items was very high in Group I, the Pearsonian correlation coefficient being $+.99$ as between the first and the second set of judgments.

Twelve of the experimental items, then, were persons and organizations attacked. Table 1 shows that there was a downward shift in opinion in Group III, which heard the newspaper materials read, on ten of these twelve items, as compared with only five downward shifts in Group I, and only three downward shifts in Group II. Also, in every instance of any downward shift in either Group I or Group II, the corresponding downward shift was even greater in the case of Group III. Although the critical ratios $\left(\frac{\text{difference}}{\text{standard error of difference}}\right)$ for the change in opinion in Group III from the first to the second questionnaire were, in general, not large, they were greater than those for Group I in eight of these ten cases of downward shifts, and larger than those in Group II in nine instances. In the case of the American League for Peace and Democracy and that of Earl Browder, the downward shifts in Group III were quite significant statistically.

TABLE 1

AVERAGE CHANGES OF OPINION FROM FIRST TO SECOND QUESTIONNAIRES WITHIN EACH GROUP

EXPERIMENTAL ITEMS	CHANGES (CRITICAL RATIOS IN PARENTHESES)		
	GROUP I	GROUP II	GROUP III
A. Persons and Organizations Attacked			
1. American Federation of Labor	+.04 (.18)	+.11 (.46)	+.12 (.46)
2. American League for Peace and Democracy	−.23 (1.21)	−.39 (1.44)	−1.28 (4.54)
3. Earl Browder	+.06 (.30)	−.15 (.56)	−.62 (2.70)
4. The C.I.O.	+.09 (.33)	+.04 (.11)	−.08 (.27)
5. The Communist Party	+.21 (1.17)	.00 (.00)	−.08 (.42)
6. German-American Bund	+.09 (.53)	+.07 (.39)	−.04 (.27)
7. International Labor Defense	−.18 (.95)	+.13 (.62)	−.32 (1.46)
8. Fritz Kuhn	−.13 (.72)	+.03 (.17)	−.18 (.95)
9. John L. Lewis	+.19 (.68)	+.04 (.12)	+.30 (1.03)
10. Mrs. Eleanor Roosevelt	−.04 (.24)	+.13 (.57)	−.06 (.26)
11. President Roosevelt	−.01 (.06)	−.04 (.19)	−.06 (.21)
12. Harry F. Ward	+.03 (.43)	+.13 (1.18)	−.16 (1.33)
B. Three Members of the Dies Committee			
13. Representative Martin Dies	−.08 (.31)	−.08 (.26)	−.42 (1.40)
14. Representative Parnell Thomas	−.12 (1.09)	.00 (.00)	−.02 (.18)
15. Representative Jerry Voorhis	−.06 (.50)	−.13 (.87)	−.06 (.50)

Table 1 also shows that Chairman Martin Dies himself went down in the opinion of the members of Group III to a greater extent than in the other two groups, and the critical ratios suggest that this downward shift in Group III is somewhat significant statistically. That no marked changes were found for Representatives Thomas and Voorhis is no doubt due to the fact that they were mentioned on only four days each, and then only incidentally in connection with the hearings, whereas the name "Dies" naturally was repeated again and again in each newspaper account on every day that newspaper stories were read aloud.

Table 2 shows the critical ratios for the shift in opinion on each item in Group III as compared with the same item in Group I; the critical ratios

between the shifts in Groups III and II; and the critical ratios between the shifts in Groups II and I. The plus and minus signs indicate the direction of the shift in the first-named group, in each column. The downward shifts in opinion were statistically significant in the cases of The American League for Peace and Democracy and of Earl Browder, as was also shown in Table 1. This is indicated in Table 2 *both* for Group III compared with Group I (first column) and for Group III compared with Group II (second column). The shifts in Group II in comparison with the Shifts in Group I (third column), however,

TABLE 2

RELIABILITY OF THE SHIFTS IN OPINION BETWEEN GROUPS

EXPERIMENTAL ITEMS	DIRECTION OF CHANGE AND CRITICAL RATIOS		
	Shift in Group III as compared with shift in Group I	Shift in Group III as compared with shift in Group II	Shift in Group II as compared with shift in Group I
A. Persons and Organizations Attacked			
1. American Federation of Labor	+.60	+.29	+.29
2. American League for Peace and Democracy	−4.23	−3.17	−.60
3. Earl Browder	−3.05	−2.33	−1.20
4. The C.I.O.	−.86	−.87	−.06
5. The Communist Party	−1.53	−.13	−1.31
6. German-American Bund	−.28	−.43	+.06
7. International Labor Defense	−1.05	−2.62	+1.84
8. Fritz Kuhn	−.37	−1.11	−1.00
9. John L. Lewis	+.82	+1.43	−.37
10. Mrs. Eleanor Roosevelt	+.32	−.21	−.67
11. President Roosevelt	+.20	+.94	−.82
12. Harry F. Ward	−1.00	−1.93	+1.31
B. Three Members of the Dies Committee			
13. Representative Martin Dies	−2.16	−1.42	−.17
14. Representative Parnell Thomas	+.79	−.13	+1.18
15. Representative Jerry Voorhis	+.67	+.79	−.25

were less significant in general than the shifts of Group III as compared with Group I. Apparently the opinions of the casual newspaper readers were not affected as consistently or to the same extent as were the opinions of the persons (Group III) who were bombarded with propaganda day after day over a period of several weeks. It should be noted, however, that *some* change was produced even among the casual newspaper readers in the space of five weeks; perhaps, therefore, the continued newspaper publicity given the Dies Committee over a much greater period (two years) has had considerable effect.

DISCUSSION OF METHODOLOGY

In addition to the specific results obtained with reference to the effect of the newspaper publicity given the Dies Committee's hearings, this experiment represents a methodological advance in realistic opinion-experimentation in two important respects.

(1) It establishes the feasibility of *prediction* in planning experiments in the field of propaganda and public opinion. Prediction, always something of a gamble, has a good chance of success if it is based on careful observation. In the present instance, two types of predictions were made some weeks before the beginning of the experiment—(a) when and how long the Dies Committee would meet, and (b) what names would be under attack in the hearings. As to the first prediction, it seemed probable that the Dies Committee would not function in the early spring of 1939; hence, the questionnaire was given to the control group during that period. It was likewise probable that the Committee would be in session during the fall; hence, this time was selected in advance for the experimental period. The second prediction involved the names of people and organizations likely to be under discussion in the hearings of the Dies Committee. On the basis of its past record, it was fairly certain that alleged Communist and Fascist groups and also liberal organizations would come under attack. A list of names from these groups was compiled, therefore, large enough to include at least some of the persons and organizations discussed at the hearings.

(2) The success of these predictions made possible a second contribution of this experiment—namely, the controlled measurement of changes in opinion on a controversial issue in response to a sequence of actual newspaper stories *exactly as they appeared in the press, and almost immediately after they had appeared*. This is to be contrasted with the method employed in some change-of-opinion studies, where a highly selected mass of propaganda material is presented to a group of subjects at a single sitting, and questionnaires are administered immediately afterward. Although such a "shotgun method" may be useful in indicating the probable *direction* of change in opinion, the present type of experiment is much more realistic, since it closely approximates the incidence and spacing of propaganda in real life. The present method differs from that of the well-known experiment by Annis and Meier (1), in that they "planted" editorials in a university daily—that is, the editorials did not appear in the regularly circulated issues—whereas in the present study newspaper stories from a metropolitan newspaper were used as they actually were written, shortly after their appearance in print.

Similar methods could well be used in other experiments dealing with the effects of propaganda. For example, such a technique would be useful in studying the effects of publicity given to any Congressional committee or governmental agency, or in measuring the reliability of the shift in opinion produced by political campaigning. The results thus obtained would not only give an indication of the direction of change in opinion, but also of the extent of change, and would certainly approximate actual situations more accurately than does the traditional type of experiment.

SUMMARY

Changes in opinion on persons and groups attacked by the Dies Committee in 1939 were measured over a period of approximately five weeks in: control Group I *prior to* Committee hearings; control Group II *during* the period of the hearings; experimental Group III, to whom were read complete newspaper accounts of the hearings just after publication of the stories. The downward shifts in opinion were both more numerous and greater in the experimental than in the control groups, and practically all the critical ratios of these shifts

were larger. Statistically significant downward shifts were found for certain items.

The investigation represents a methodological advance in realistic opinion-experimentation in two important respects. First, the study establishes the feasibility of *prediction* in planning experiments in propaganda and public opinion. Predictions were made (a) as to when and how long the Dies Committee would meet, and (b) as to what names would be under attack in the hearings. Second, the experiment demonstrates the use of controlled measurement of opinion change on a controversial issue in response to actual newspaper stories *exactly as they appeared in the press, and almost immediately after they appeared.*

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THE RACIAL STEREOTYPES OF NEGRO COLLEGE STUDENTS*

BY JAMES A. BAYTON
Virginia State College

MOST studies of racial attitudes seem to direct their attention to the investigation of the attitudes of the native white American population toward minority racial elements and other racio-national groups. There seem to be too few studies from the opposite direction, namely, the investigation of the attitudes of the minority and other racio-national groups toward the native white population and toward each other. In other words, more work needs to be done on the attitudes of Negroes, Jews, Chinese, and like groups, toward white Americans, toward other minorities, and toward themselves. The following paper reports one such attempt in this particular field.

Procedure

The technique used by Katz and Braly (1) in a previous study was chosen. A questionnaire was given to 100 Negro college students, 80 per cent of whom are from southern states. In this college all students, faculty, and administrative officers are Negro. The questionnaire listed eighty-five adjectives covering several degrees of desirability. The subjects were asked to select any adjective which they thought typical of the following groups: white Americans, Jews, Negroes, Germans, English, Irish, Italians, Chinese, Japanese, and Turks. No limit was placed on the number of adjectives to be selected. When the ten listings had been completed, the subjects were instructed to go back over their lists and to check the ten traits most characteristic of the respective races.

One month after taking the first questionnaire the same students were given the same list and instructions, with the exception that now they were to characterize the typical student at their college. In order to check on the effect of answering the first questionnaire, a control group of 100 was given the same instructions.

Results

Permission has been obtained to compare the results with those reported by Katz and Braly (1) who used 100 Princeton men as subjects. The results will be presented by means of a direct comparison, in terms of similarities and differences, between the two studies. Reference will also be made to factors revealed by the frequencies with which the traits were assigned.

Negroes. The two groups were in agreement on the following traits: superstitious, lazy, happy-go-lucky, very religious, ostentatious, loud, and musical. They differed in that whites assigned to Negroes the adjectives ignorant, stupid, naive, slovenly, and physically dirty; whereas Negroes listed intelligent, progressive, faithful, and imitative.

Although there is definite agreement on one grouping of traits, there are some choices which are direct opposites. Whites assign ignorant and stupid;

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TABLE 1

RACIAL STEREOTYPES OF 100 NEGRO COLLEGE STUDENTS

TRAITS	NUMBER	TRAITS	NUMBER	TRAITS	NUMBER
Negroes		*White Americans*		*Jews*	
musical	63	intelligent	63	progressive	38
superstitious	62	industrious	37	shrewd	34
very religious	53	scientifically-minded	36	ambitious	32
happy-go-lucky	38	progressive	28	grasping	27
loud	32	ambitious	25	industrious	27
lazy	25	peace-loving	21	very religious	25
progressive	24	sportsmanlike	18	intelligent	23
imitative	21	sophisticated	15	deceitful	22
intelligent	20	conceited	14	brilliant	16
faithful	17	neat	13	loyal to family ties	15
Germans		*English*		*Irish*	
scientifically-minded	78	intelligent	61	tradition-loving	28
intelligent	52	sophisticated	34	honest	23
ambitious	35	sportsmanlike	29	peace-loving	23
extremely nationalistic	25	courteous	27	jovial	21
cruel	21	reserved	24	witty	20
aggressive	17	tradition-loving	23	quick-tempered	20
industrious	17	loyal to family ties	22	musical	18
brilliant	15	industrious	21	straight-forward	15
revengeful	15	conservative	20	intelligent	13
shrewd	15	peace-loving	19	industrious	12

TABLE 1—*Continued*

TRAITS	NUMBER	TRAITS	NUMBER	TRAITS	NUMBER
Italians		*Japanese*		*Chinese*	
musical	42	tradition-loving	24	superstitious	38
physically dirty	35	ambitious	22	tradition-loving	37
intelligent	25	progressive	20	loyal to family ties	29
artistic	17	intelligent	18	physically dirty	27
ambitious	15	shrewd	18	quiet	25
passionate	15	industrious	17	sly	24
quarrelsome	15	extremely nationalistic	16	ignorant	14
quick-tempered	15	loyal to family ties	15	stupid	14
scientifically-minded	14	pugnacious	15	stubborn	11
loud	12	superstitious	14	courteous	11
		Turks			
		treacherous	20		
		shrewd	19		
		tradition-loving	18		
		sly	17		
		superstitious	16		
		very religious	16		
		cruel	15		
		pugnacious	13		
		revengeful	13		
		quick-tempered	12		

Negroes assign intelligent. White assign unreliable, Negroes faithful. There is also one instance of inconsistency within the stereotype as given by Negroes: they list both lazy and progressive.

White Americans. Both groups agree on the following adjectives: industrious, alert, efficient, ambitious, progressive, intelligent, nationalistic, scientifically-minded, pleasure-loving, and sportsmanlike. The whites differed in listing aggressive and straightforward, while Negroes listed conceited, neat, and sophisticated.

Jews. The stereotype common to both groups was as follows: shrewd, grasping, sly, mercenary, aggressive, persistent, industrious, intelligent, ambitious, progressive, very religious, and loyal to family ties. However, whites listed talkative, whereas Negroes listed deceitful.

Germans. The similarity between the two groups is shown by their both listing the following: scientifically-minded, nationalistic, progressive, ambitious, persistent, and aggressive. Differences are seen in the whites' listing of stolid, methodical, jovial, and musical, as opposed to the Negroes' cruel, revengeful, and shrewd. This difference is apparently due to the fact that the Princeton study was made in 1933.

English. The two groups gave the following similar characterization: sportsmanlike, intelligent, industrious, conventional, conservative, reserved, tradition-loving, loyal to family ties, sophisticated, and courteous. The groups differed in that the whites listed honest, extremely nationalistic, and humorless, while Negroes listed peace-loving. This difference could also have been caused by the date on which the Negroes took the questionnaire.

Irish. The groups agreed in listing: quick-tempered, witty, jovial, honest, straightforward, and industrious. The whites listed very religious, superstitious, stubborn, and imaginative, whereas Negroes listed peace-loving, tradition-loving, musical, and intelligent.

Italians. Similarity between the two groups is seen in the following adjectives chosen: artistic, musical, passionate, quick-tempered, quarrelsome, physically dirty, talkative, and loud. The whites listed imaginative, very religious, revengeful, lazy, and unreliable, and the Negroes listed intelligent, ambitious, and scientifically-minded.

Japanese. Both Negroes and whites listed intelligent, ambitious, industrious, progressive, shrewd, sly, and treacherous. Whites differed by listing quiet, aggressive, suave, and neat, while Negroes listed tradition-loving, extremely nationalistic, pugnacious, and superstitious.

Chinese. Similarity is seen in both groups, listing superstitious, sly, tradition-loving, conservative, loyal to family ties, meditative and quiet, ignorant, stupid, deceitful and sly. Whites listed industrious and very religious; Negroes listed physically dirty, stubborn, and courteous.

Turks. Both groups listed cruel, treacherous, deceitful and sly, shrewd, revengeful, quick-tempered and quarrelsome, superstitious, very religious, conservative and tradition-loving. The Princeton group listed sensual, ignorant, and physically dirty, while the Negro group listed pugnacious.

Negro college students. The experimental and control groups agreed on the following adjectives: talkative, intelligent, happy-go-lucky, sportsmanlike, jovial, musical and impulsive. The experimental group, however, listed radical, sophisticated, and generous, whereas the control group listed conceited, ambitious, and imaginative. It should be noted that the Princeton students were not asked to characterize the typical Negro college student.

Frequencies. The frequency with which a trait is assigned to a race indicates how widely that opinion is held. Seventy-eight subjects in each study assigned "scientifically-minded" to Germans. The frequencies, however, also show that some of the specific traits assigned to a race by both groups did not have equal weight in the total stereotype. Sixty-three Negro subjects assigned "musical" to the Negro, but only twenty-six whites made this characterization, a circumstance which indicates that the notion of Negroes as musical is more widely held among Negroes than among whites.

TABLE 2

STEREOTYPE OF THE TYPICAL VIRGINIA STATE COLLEGE STUDENT

TRAITS	NUMBER	TRAITS	NUMBER
Experimental Group		*Control Group*	
talkative	51	intelligent	48
intelligent	50	happy-go-lucky	47
happy-go-lucky	49	talkative	47
sportsmanlike	41	sportsmanlike	40
jovial	40	musical	35
radical	32	ambitious	32
sophisticated	31	impulsive	20
generous	30	jovial	19
impulsive	28	imaginative	15
musical	24	conceited	14

DISCUSSION

It is evident that the two groups of college students, Negro and white, exhibit a high degree of similarity in the stereotypes they possess of the various racial and national groups. One important aspect of the results is that the Negro college students, when asked to assign traits to "the Negro" without any qualifying statement, were not greatly influenced by the traits which they consider themselves to have. That they were, perhaps, influenced to some degree is seen in the fact that they did assign intelligent, musical, and happy-go-lucky in both instances. Thus in American culture, where definite characterizations are assigned to the various racial elements, we see the Negro accepting these "typings," including much of the one assigned to himself.

There are two ways by which stereotypes are acquired. One method is through propaganda (1; 2, 82; 3, 1040–1045). Without necessarily having malicious intent, we turn to symbolic classifications whenever it is necessary to portray a certain race or national group. It is now an old story that our concept of the Turk is due solely to the roles given him in literature, the movies, and in legend.

In the second place, our stereotypes must be affected, to some degree, by our personal contacts. If our personal, "face-to-face" contacts are continually of given qualities, the resultant stereotype should reflect these qualities. In the final analysis stereotypes must be a synthesis of these two influences—propaganda and personal contact. The problem then arises as to which of these is the more influential in this situation. (Of course in such cases as the Turk personal contact must be ruled out of consideration except for isolated instances.) Contact can either be operating to bolster the information received through propa-

ganda, or to offset it. If one meets individuals who conform to the usual ways of picturing that person's race, obviously the stereotype will be further enhanced. But if the two influences offer contesting elements, the question arises as to which will win out in terms of differential influence.

One part of the present study seems to approach this particular problem. The traits these Negro college students assign to themselves must have been derived on the basis of their personal contacts with their fellow-students. What, then, is the source of their very different stereotype of the typical Negro? In their present environment their contacts are with individuals who do not possess in wholesale fashion the usually assigned negroid traits. Those having these traits in college are decidedly the exception rather than the rule (a feature characteristic of similar environments regardless of race). Then, too, it is probable that the home backgrounds of these students were not characterized by qualities supposedly "typical" of the Negro.

So we seem to have the following situation. A group of Negroes is living in close personal contact with other Negroes. The things which impress them as being most characteristic of their own college group are traits which are decidedly different from those they think characteristic of typical Negroes. Also, if the supposition is permitted, they have not been exposed to the latter qualities in the personal contacts in their home backgrounds. At the same time that this close personal association is going on, these students are also being exposed to a very different influence—propaganda. In literature, the movies, cartoons, jokes, the radio, and the theatre, they see the Negro portrayed as stupid, lazy, superstitious, and the like.

That both influences contribute to their final stereotype of Negroes in general is seen in the mixture of traits assigned. In spite of choosing many qualities similar to those obtained from white subjects, they did insert such an item as "intelligent" as characteristic of the typical Negro. The fact that propaganda, however, has exerted a much greater influence than personal association is seen in the ranking of the traits. In the stereotype of the typical Negro, intelligence is ninth in the list. Superstitious, very religious, loud, and lazy are second, third, fifth, and sixth, respectively.

We have, then, an indication of the subtleness with which propaganda becomes effective. Here is a group accepting what has been called the "group fallacy." Also, they accept, without contradiction, the stereotypes assigned other minorities as shown in their characterization of Jews. Moreover, they largely accept the stereotype usually assigned to themselves, in spite of the fact that their everyday contacts seem to be with Negroes of different qualities.

CONCLUSIONS

1. Negro college students have racial stereotypes highly similar to those possessed by white college students.

2. Negro college students assign characteristics to themselves which are different from those assigned to the "typical Negro."

3. Propaganda is more influential in the formation of stereotypes than are personal "face-to-face" contacts.

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TRAUMATIC AMNESIA DURING HYPNOSIS

BY MARVIN LIEBMAN
Yale University

THE author herewith presents what he believes to be the first reported case of an amnesia developing during hypnosis as the result of a head blow during the trance. Erickson has described the development of apparent unconsciousness during the hypnotic reliving of a traumatic experience in which the subject was drugged into unconsciousness; [1] but a careful search of the literature, including all the articles listed in the *Psychological Abstracts* from 1927 through the most recent issue, fails to reveal any previous report of amnesia developing during hypnosis following a head injury.

The subject was a cooperative young college student who had been hypnotized on several occasions by the experimenter. He always entered into complete somnambulism and carried out suggestions perfectly; likewise he always awakened quickly and with no ill after-effects. The present occasion was a demonstration of hypnosis before several witnesses. After the subject had passed into somnambulism, the experimenter proceeded to have him go through a series of performances.

The whole performance was conducted with the subject lying or sitting on a bed. The subject was first asked to raise his arm, then to throw a "stone," to sign a song, to play a "saxophone," to recite "Jack and Jill." Next he was told that it was freezing in the room, whereupon he began to shiver violently. Finally the experimenter suggested that it would become warm, then warmer, then oppressively hot. The subject began to breathe heavily, to unbutton his collar, to writhe and roll uncomfortably on the bed. In one of these rolling movements, he struck his head sharply against the corner of the windowsill. It was a hard blow, and the subject evinced pain, continuing to rub his head for some time. We continued with the demonstration, however, and the subject was just as cooperative as usual.

But when the experimenter proceeded to awaken the subject, he was unresponsive. The experimenter repeated the suggestion; still no response. With more powerful suggestions, the experimenter succeeded in awakening the subject. But he soon closed his eyes and dropped back to the hypnotic state. The hypnotist again awakened him, and by strong commands brought the subject back to an apparently normal waking state. The subject then walked out of the room, not speaking a single word in all this time. The experimenter and witness followed him. Suddenly the subject's knees wobbled, and we caught him just as he collapsed to the floor. The hypnotist again awakened him and brought him back to the bed.

Wishing to make sure that the subject was oriented, he asked: "Milton [the subject's name], you know where you are, don't you?"

"No," replied the subject, staring blankly.

"Why, you're in the room," the hypnotist assured him.

"What room?" asked the subject.

[1] Erickson, M. H. Development of apparent unconsciousness during hypnotic reliving of a traumatic experience. *Arch. Neurol. Psychiat.*, Chicago, 1937, 38, 1282.

The hypnotist explained to him that he was in the bed, in this room, and that we had agreed to give a demonstration here. The subject listened uncomprehendingly.

Then, "*Who am I?*" he asked.

"Why, you're Milton A——."

"I don't know him. Who is he?" queried Milton.

Subsequent questioning showed that the subject had an apparently complete amnesia for his name and other facts, including even events which had transpired during the trance.

Such a phenomenon had never before occurred in the experimenter's experience. Fearing that something he had said had inadvertently suggested amnesia, the hypnotist decided to start from the beginning and reconstruct and recall the events of the trance, in order to find the cause of the amnesia. The hypnotist told the subject to listen while he narrated the events leading up to the subject's lying down in the bed to be hypnotized, and told him that he would remember everything. He then recalled his lying down and his going to sleep.

Telling the subject that he had been asked to raise his arm, the hypnotist asked if he remembered. He said he did.

"Then I told you to throw a stone. You recall, don't you, Milton?"

"Ye-e-s," he replied.

"Then you sang a song. Remember?"

"Ye-e-s."

"Then you played a saxophone. Remember?"

"Yes, I remember."

"Then you remember you recited 'Jack and Jill?'"

"Yes."

"Then I told you it was freezing, and you shivered, didn't you, Milton?"

"Yes, I was cold."

"Then it became very hot in the room. You were just roasting. You remember."

"I think so."

"You remember we decided to go in swimming?"

"*No, I don't remember,*" replied the subject.

From that point onward, he remembered nothing that had transpired during the hypnotic trance—nothing whatever. He just began rubbing his head with his hand as if trying to recall something. All of the witnesses recalled that just at this point where his memory failed, he had bumped his head on the windowsill. He recalled all events up to the suggestion that it was very hot, whereupon he had begun to roll in the bed and struck his head; but after that he remembered nothing. Now, moreover, he rubbed his head just as he had after receiving the bump.

The experimenter continued the questioning:

"Yes, you took off your shirt and shoes and socks as I suggested, didn't you?"

"My shoes and socks? I don't know."

"Yes, you went into the 'water' at my suggestion, and when I asked you how it felt, you said, 'Cold.'"

"I remember it was *hot,*" replied Milton.

"Yes, that was when I said it was hot in the room, roasting. But you said the *water* was *cold,*" rejoined the experimenter.

"I don't remember," he continued without expression.

"Then you showed me how you swim the Australian crawl, didn't you?"

"I don't know. What is the crawl?"

"Why, it's a swimming stroke."

"Swimming? What is swimming?"

"It's an exercise in the water."

"Oh," drawled the subject, and continued to rub his head.

Seeing that this kind of dialogue might lead to endless digressions, the experimenter discontinued his questions at this point. Not only was the subject's memory apparently blank for the whole "swimming" episode, but he seemed likewise to have forgotten the connotation of common words connected with the episode, such as "crawl" and "swimming."

Here was, then, an apparently clear-cut case of an amnesia resulting from a head blow during hypnotism. It was, moreover, an amnesia apparently more complete for events *after* than before the shock.

Finding that the amnesia began at this point, the experimenter re-educated the subject by recounting everything that had transpired since the head injury, *insisting* that he should and would remember everything. Each event was recounted until the subject definitely said, "Yes, now I remember." Finally he remembered where he was and awoke, his memory quite refreshed and normal. No apparent organic lesion was found—not so much as a laceration of the scalp. Since this experiment, the subject has entered one of the foremost dental schools in the country, and stands at the top of his class.

While this is the first case of its kind reported in the literature, the circumstances clearly indicate that it is a case of amnesia resulting from a head injury *during hypnosis*. That it was a functional amnesia is shown by the fact that memory was quickly re-established—as would probably not have been possible if a part of the brain had actually been injured.

Note: The ordinary conception of amnesias, such as those resulting from repressions (functional) or from head injuries (traumatic), is that they result from the relegation of ideas from the *conscious* to the *unconscious*. Since hypnotism is supposed to be a delving into the unconscious, hypnotism should be a means of curing such amnesias. McCartney states that it is "the best method for the treatment of amnesias," [2] and Smith reports the cure by hypnosis of a phobia originating before the age of three.[3] These studies indicate that the amnesia does not go below the hypnotic state.

The case herein reported seems to indicate that ideas may be relegated from the conscious even *beyond the unconscious*. Since the amnesia occurred during the ascendance of the hypnotic (unconscious) state, there would seem to be an *ultra*-unconscious state existent. This may be the state entered into by Erickson's subject during hypnosis.

Such a theory may also explain why material apparently completely forgotten is nevertheless more easily learned the second time.

[2] McCartney, J. L. Hypnosis: a rational form of psychotherapy in treatment of the psychoneuroses. *J. nerv. ment. Dis.*, 1937, **86**, 405.

[3] Smith, G. M. Phobia originating before age of three cured with aid of hypnotic recall. *Psychol. Bull.*, 1937, **34**, 521.

A POSSIBLE DYNAMIC EXPLANATION OF VARIABILITY IN THE I.Q.*

BY LEOPOLD BELLAK
Cambridge, Mass.

ACCORDING to psychoanalytic theory, intelligence in its various aspects bears a certain relationship to libidinal energy, depending at least in part upon the state of gratification or frustration of direct instinctual demands and the amount of free libidinal energy available.

Psychoanalysis also teaches that an early flowering of infantile sexual life takes place between the second and the fifth year, followed then by the latency period that is characteristic only of human development. This latter is supposedly due to the influence of some prehistoric experience of *homo sapiens*, when climatic changes resulted in impairment of living conditions and necessitated instinctual renunciations. Whatever the biological explanation may be, the important point for us is the fact that the sexual development in man takes place in two periods: the early infantile one and the one generally called puberty, which comes after the period of latent sexuality.

According to this assumption, the growth of intelligence should be markedly influenced by these libidinal changes. Presumably an increase in the rate of growth of intelligence would take place in the latency period, becoming effective after the fifth year, since the phylogenetic frustration should make a greater amount of libidinal energy available for intellectual processes.

The question now arises, whether or not evidence can be found to support this hypothesis by measurements. A preliminary difficulty is to arrive at a useful conception of intelligence, reconciling various viewpoints.

The psychoanalytic position holds that during the latency period the intellectual machine is used with greater energy than hitherto because of the greater amount of libido which is poured into intellectual processes. This increased intelligence shows itself in greater alertness, inquisitiveness, and in an increase of general interest. At the same time it is claimed that the I.Q., for instance, measures the intellectual *motor* rather than the *amount* of *energy* that is poured in, and thus measures something that is different from the psychoanalytic conception of intelligence.

I do not, to be sure, think this viewpoint acceptable. We are not able to measure these two components, the efficiency of the motor and its energy, separately; all we ever can perceive is a certain intelligence as the result of both. An analysis of the Stanford Binet Test, for instance, will show that it consists of many tests for various special abilities and achievements—memory ability, problem-solving ability, or general information, and so forth. I should think it is evident without further investigation that memory ability will always be a function of both the "brain capacity" and the interest taken in the task (i.e., the libidinal energy which is poured into the process), and that just the same is true in the case of problem-solving or any other ability. It may be said that the subject's whole attitude toward the test—willingness and attention,

* The author is indebted to Dr. L. S. Kubie for his kind advice. The responsibility for statements contained herein is, however, the author's alone.

known to be of tremendous importance for the result—should be influenced by the characteristics of the latency period.

We may be justified, therefore, in assuming that what psychoanalysis means by intelligence and what the intelligence test measures are closely linked together, and we can proceed with our investigation of the rate of increase of intelligence.

But there are further difficulties with the tests themselves. The methods generally used at present for the measurement of intelligence do not permit us to make statements regarding the rate of increase of intelligence between two given age levels. The Stanford Binet Tests, for instance, do not indicate whether intelligence develops regularly and steadily, or whether a child's intelligence increases, say five per cent from the ages three to four, and twenty-five per cent from the ages five to six. (These figures are purely schematic, and presuppose that the grade of intelligence ultimately reached is considered to be one hundred per cent.)

These tests do, however, allow us to say that I.Q. scores are fairly constant throughout the life of an individual. That means that a child's endowment in intelligence is expressed in the I.Q. as a measure of the standing of his individual intelligence as compared with the standard (the average attainment of the rest of the children at this age level) and that it maintains this same relationship to the standard throughout his life and development. Since the I.Q. is simply an indication of such a relation with an average, and since the tests themselves are standardized on the average of a certain age level, *we learn nothing of the rate of progress in intelligence from year to year.*

There is a way out of this difficulty—if we are able to devise tests which in themselves give a quantitative indication of their increased difficulty. These requirements may be fulfilled by certain memory material, for example, digits.

Speaking schematically again, according to our assumption, a child of three years would be able to reproduce three digits: a four-year-old, four digits; a five-year-old, five digits; but a six-year-old would be able to repeat seven degits, the seven-year-old nine. A faster increase of memory capacity should be expected to take place with the onset of the latency period. (It is understood, of course, that this statement is meant schematically only, certainly not as an assertion that a child able to repeat six digits would have just twice as much intelligence as the one who would be able to repeat only three digits. A much more complicated relationship would exist.)

Besides the repetition of digits forwards and backwards, nonsense syllables could be used. A preliminary difficulty which research of this kind would meet is the finding of enough material allowing such quantitative measurement, particularly in fields other than memory. No work has yet been done along this line that would permit us to establish a favorable influence of the latency period upon mental growth. We may, nevertheless, obtain valuable evidence that the period does have a definite influence upon the development of intelligence without going into this research problem. For such evidence we have to thank the thorough reports of Terman and Merrill on *Measuring Intelligence.*

If we examine Terman's and Merrill's "Statistical Analysis of Scores" we find some informative data.[1] In the chapter, "I. Q. Variability in Relation to Age," examination of the variability of the test scores brings a result that frankly puzzles the authors. Terman and Merrill, after having pointed out

[1] L. M. Terman and M. A. Merrill. *Measuring intelligence.* Boston: Houghton Mifflin, 1937, pp. 40–41.

that there is no marked relationship between *I.Q. variability and the chronological age* over the age range as a whole, say: "Attention, however, should be drawn to the ages six and twelve, where the relatively low and high values respectively are deviations too extreme to be explained as purely chance fluctuations. The high variability at the age twelve might conceivably be ascribed to the differential age of the onset of pubescence, although it has yet to be demonstrated that pubescence is significantly related to the rate of mental growth. Whether the atypical I.Q. variability at age six resides in the sampling at that age, or whether it is perhaps an artifact of the nature of the scale cannot be determined from the available data. In the lack of positive proof to the contrary, we are probably justified in assuming that the true variability is approximately constant from age to age."

TABLE 1

I.Q. VARIABILITY IN RELATION TO AGE [2]

C.A.	N.	SIGMA L.I.Q.	SIGMA M.I.Q.
2	102	16.7	15.5
2½	102	20.6	20.7
3	99	19.0	18.7
3½	103	17.3	16.3
4	105	16.9	15.6
4½	101	16.2	15.3
5	109	14.2	14.1
5½	110	14.3	14.0
6	203	12.5	13.2
7	202	16.2	15.6
8	203	15.8	15.5
9	204	16.4	16.7
10	201	16.5	15.9
11	204	18.0	17.3
12	202	20.0	19.5
13	204	17.9	17.8
14	202	16.4	16.7
15	107	19.0	19.3
16	102	16.5	17.4
17	109	14.5	14.3
18	101	17.2	16.6

It is particularly noteworthy that Terman and Merrill should have discovered a result which approximates so closely psychoanalytic theory—an agreement which lends added strength to the psychoanalytic interpretation.

May I also point out—what Terman and Merrill forgot to mention—that there is *one other time* between the year two and the year eighteen where the variability is extraordinarily high; and that is at the age of two and a half years (sigma 20.6), approximately the time at which *psychoanalysis places the onset* of the *first flowering* of sexuality? At the *three* age-levels claimed by psychoanalysis to be noteworthy in the child's development, we find extraordinary variability in Terman and Merrill's scales! The same changes hold true for the variability in both the M. scale and the L. scale, thus excluding the possibility of artifacts of the scales or sampling.

[2] *Ibid.*, p. 40. Reproduced by permission of and special arrangement with the publishers, Houghton Mifflin Company.

Not only is the high variability in I.Q. at the onset of pubescence explained by the psychoanalytic theory of the relationship between libidinal and mental processes, but also the *low variability at six* is apparently explained satisfactorily as the result of the *uniform phylogenetic onset* of the *latency period*, and of the uniform changes at that time which are marked enough to rule out individual differences.

The assumption is that the sum total of sexual libido and desexualized, sublimated libido (which is of use in intellectual processes) in one person is constitutional and always equal, whereas the distribution of either changes. Also, there are individual differences in the distribution of either kind of libido among children of the same age.

The onset of the latency period must be understood to be the beginning of a biological process which Freud claimed to have observed empirically. Such a process is characterized by an increase in the sublimation of sexual libido. The gain in sublimated libido is much greater in children who previous to the onset of the latency period have had relatively little sublimated libido and much sexual libido. The gain in sublimated libido is greater in these children, because it is necessitated as an adjustment to the increasingly difficult problems of reality which the child has to face and because the force of the onset of the latency period utilizes the maximum energy which is available without disturbing the fundamental emotional relations. (For the sake of simplicity we neglect wholly the process of repression, using again a schematized, simplified picture.) The *result* of the *equalizing processes*, a *nearer approach to the average*, accounts for the *low variability* that we find.

The high variability at the year twelve is best understood from the explanation given by Terman and Merrill, namely the result of the onset of pubescence. There we apparently find the maximum variation in stages of libidino-intellectual development, the one still more or less in the latency, the others more or less in puberty.

It might seem arbitrary to claim that any of these developmental periods sets in at precisely two and a half, six, or twelve years, respectively. But we should remember that the data of Terman and Merrill are based on a large number of subjects, thus giving us the *average* onset of sexual flowering or latency, which is all that is claimed. Note that the variability is not only at a peak at precisely two and a half years (see table: sigma 20.6), but still is high at three years (sigma 19.0); that it not only is very low at precisely six years (sigma 12.5), but also is now at five years (sigma 14.2) and at five and a half years (sigma 14.3). The variability is not only very high at twelve years (sigma 20.0), but is high also at eleven (sigma 18.0) and at the year thirteen (sigma 17.9). The same principles in variability hold true both for the L. and M. scales.

THE WORK CURVES OF PSYCHOTIC INDIVIDUALS*

BY NOEL MARIE MAILLOUX AND MAURICE NEWBURGER
University of Cincinnati

THE present study was carried out in order to find whether or not continuous response to color stimuli might be valuable in determining the degree of mental disorganization in psychotic individuals. The experimenters were interested in comparing the rate and distribution of responses and blocks in psychotic and normal individuals during a 10-minute period of work.

One hundred and thirteen patients in the Longview State Hospital in Cincinnati were employed as subjects for the experiment. Equally divided on the basis of sex, they ranged in age from 18 to 75 years. They were all of relatively recent admission to the hospital and were obtained from the receiving wards without regard to their diagnoses. As a rule, the patients who worked were cooperative, apparently motivated by a common desire to demonstrate their abilities. For purposes of control, the results obtained were compared with those obtained by Bills in a similar study involving normal individuals.

The color-response apparatus which was used proved to be highly satisfactory; it was of the type constructed by Vaughn and Strobel, the description of which will appear in an early issue of the *Journal of General Psychology*. It is made up of three parts: an exposure device having an opaque window behind which are four differently colored electric light bulbs; a keyboard with four finger keys, each one of which corresponds to a single light; and a chronograph.

The experiment was conducted during summer months on a large, well-lighted, screened porch some distance away from the wards on which the patients were kept. The apparatus was placed on a table before which the patient sat in a comfortable chair. Having been shown the window through which he would see the colored lights, he was told to press the key of the same color as that of the first light presented. By this action he would cause the light to disappear and at the same time a second light to appear. The operator then demonstrated the procedure, and when it was certain that the patient understood what was expected of him, he was allowed five minutes in which to practice the required operations. Immediately following this period, a time mark was drawn on the chronograph tape and the patient instructed to continue working until told to stop. In many cases, it was necessary repeatedly to encourage the patient in order to have him continue throughout the work period. While one of the patients was carrying out the procedure, one of the experimenters worked in close proximity to him; the other one went about the ward, talking to the other patients in order to prepare them for the part which they were later to play in the experiment. This seemed to be a very satisfactory method of obtaining the cooperation of the patients.

When all of the subjects had completed their work, the records were examined. Then the diagnosis of each patient was obtained from information provided by the medical staff of the hospital, a step taken in order to find whether any relationship could be determined between the nature of the psychosis of an individual and the appearance of his work curve.

Upon examination of the rates of response, it became apparent that each patient might be placed in one of six well-defined groups. Fifty-five of the

* We are indebted to Dr. A. G. Bills for his coöperation and to the staff of the Longview State Hospital, who made available to us the facilities of their hospital throughout the experiment.

total number responded more frequently than 300 times during the 10-minute period. We choose to call these patients "rapid workers" in contrast to the 15 "slow workers" who required more extensive training and who responded less frequently than 300 times during the same period. In addition, these latter patients required encouragement and persuasion throughout their work. Eight patients learned about as rapidly as the slow workers and in general exhibited the same degree of efficiency, but differed from them with respect to the uniformity of their work. Even though these patients were given repeated instructions and had the same advantage of practice, their work records show periods of relatively slow but regular responses followed by periods of apparently random activity.

Nine subjects made real attempts to carry out the instructions but were unable either to execute them or to repeat a demonstration of them. These men and women were permitted to work for 15 minutes, but in spite of their repeated efforts they showed no improvement. Most of them depressed the keys without relation to the stimuli. This fact causes us to believe that these subjects were unable to detect any stimulus-response relationship in the light-key situation. When considered with the kinds of responses made by the members of the other groups, such a circumstance would indicate that the subject either understands the problem entirely or not at all—perhaps an operation of the "all or none law." Because we believe that the problem failed to have any meaning to these subjects, we did not make an analysis of their work curves.

The members of another group of patients were incapable of even continuing effort for longer than a few moments. All 13 of them accepted the task with great eagerness and then after looking over the apparatus began upon their task. Their interest, however, suddenly disappeared, and they refused to work further. Since they had withdrawn from the problem situation, it became necessary to remove them from the place of the experiment. Some in this group worked for as many as 4 minutes and then refused to continue. No amount of persuasion was successful in bringing about further work.

Of the entire group of 113 patients, only 11 were completely uncooperative. Some of these expressed marked suspicion, whereas others failed to demonstrate any interest at all.

In an experiment that was very similar to ours Bills found that 33 normal adults responded to color stimuli at an average rate of 72.2 times during each minute. Observation of Figure 1 discloses the fact that only one of our subjects exceeded that rate and that all of the others fell well below it. The most distinct mode appears at a point representing 52.5 responses during the same length of time. In addition, it can be readily seen that a clear line of demarcation exists between the normal and the psychotic group on the basis of the frequency of their reaction to this kind of stimulation. Another characteristic difference lies in the fact that there is much greater uniformity in the work of normal subjects.

Several earlier investigators (3) have noticed the appearance of brief periods of inactivity during otherwise continuous mental work. These periods, called "blocks," are especially characteristic of continuous and homogeneous work. They become more frequent and they increase in size during fatigue, and are apparently related to errors in response and to the competition between elements in a repetitive task.

In order to determine the presence of a block, Bills has accepted as a standard the unit of time which just exceeds twice the time required for the average

single response during a given minute. In the present study, in order to make certain that our criterion would not be inadequate, we chose as a measure of a block a period of inactivity which exceeded the average response time by not less than three times its length. Although the work curves of all of the psychotic workers demonstrated the presence of blocks, only those of the 55 most efficient subjects were chosen for comparison. In these cases, an average of more than 5 blocks occurred during each minute (actually the mean number of responses was 52.65 ± 1.73 for 10 minutes). This finding is in contrast to Bills' normal subjects who presented an average of a little more than 3 blocks during the same length of time (1).

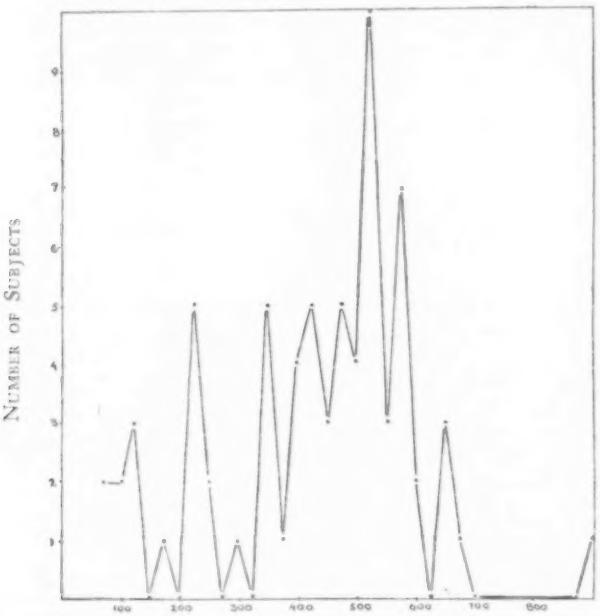

FIG. 1. DISTRIBUTION OF NUMBER OF RESPONSES DURING 10 MINUTES

The group of normal subjects employed by Bills in order to establish his norms was much like those of the psychotic group in the present study except for psychosis. Both groups were made up of men and women of many different ages and levels of education and of different occupations and economic conditions. Since the time of Bills' experiment, he has had additional subjects carry out the procedure of the experiment and now, after determining the rate of response and of blocking for several hundred adults, he finds his original results to be unchanged. This circumstance leads us to believe that the divergence in our results must be caused by some factor peculiar to the psychotic individuals. Since the normal subjects block less frequently than the psychotics and respond more frequently, it seems indicated that rate of response alone is not the factor which determines the frequency of blocks. This consideration is of theoretical importance in the question of whether or not the block represents a kind of refractory phase.

Figure 2 represents the distribution of the number of blocks for each of the psychotic subjects.

We see upon examination a highly skewed curve with a mode which falls between 30 and 40 blocks for 10 minutes. Upon further examination, it appears that only 8 cases fall below the mode, whereas 36 fall above it—a circumstance indicating a tendency in the direction of increased blocking among the psychotic subjects. Upon examination of the work records of these subjects, it was noted

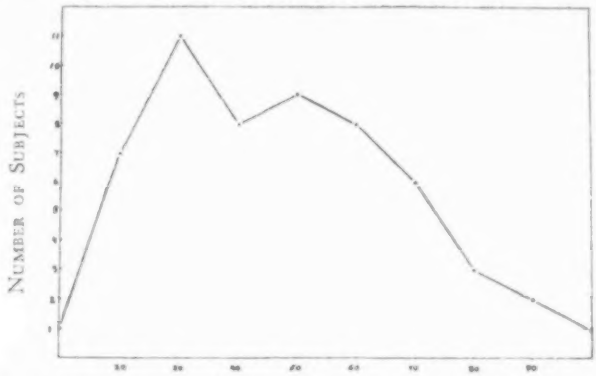

FIG. 2. FREQUENCY OF BLOCKS DURING 10-MINUTE WORK PERIOD

that there is far greater variability in the length of the blocks and that, in general, they are longer than those found to occur in normal subjects. Such a condition might be explained in terms of both increased distractibility and instability on the part of the psychotics.

Figure 3 shows that blocks occur at somewhat irregular intervals. Some

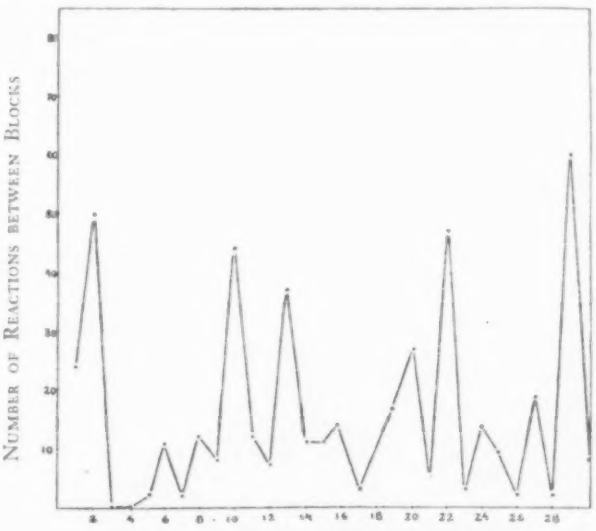

FIG. 3. RELATION OF BLOCKS TO RESPONSES

blocks follow others almost immediately, while others occur after a relatively great number of responses. In five instances during the entire work period the blocks are separated from the immediately preceding ones by more than forty responses. These peaks of efficiency take place with some degree of regularity and, in every instance except the first, follow a period during which several blocks have occurred in fairly rapid sequence. This fact indicates that in the present situation the number of responses alone does not determine the frequency of blocks, and in addition suggests that the blocks serve as a period of rest during which the subject recovers from the effects of his previous work.

Since it proved inadvisable, because of the irregularity with which the blocks appear, to combine the individual curves, the illustration represents the work of a single subject. The results in this case, however, seem typical of the group.

Except in one instance there was apparently no relationship between the work carried out by the members of any group and the nature of their psychoses. Among the 9 subjects who were unable to learn to carry out the instructions after 15 minutes, 7 were paretics and 2 schizophrenics. Since in every other group there seemed to be no relationship between diagnosis and efficiency of work, we believe that the method we have employed does not offer any way of determining the kind of psychosis from which a patient suffers. We do believe, however, that this method offers a way of determining the degree of disintegration in the mental functioning of a patient. If that be true, then such a measure of disintegration might serve as a valuable aid in the prognosis of the development of psychotic individuals under treatment.

Since the first recognition of the block in mental work, investigators have attempted to learn under what conditions blocks occur (2). Some have suggested a very close association with refractory phase; such a relation is held to account for the increased blocking during fatigue. In the present experiment, however, we find that psychotics block more frequently but respond less frequently than normal individuals—an indication that either the phenomenon of refractory phase is not the only important factor in the development of a block or else that in psychotic individuals some other factor, unknown to us at present, plays an important part in the development of the block.

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THE POSSIBLE USE OF THE MEMORY SPAN FOR THE INDICATION OF COMPLEXES

BY CLARENCE W. YOUNG
Colgate University

DURING a recent experimental investigation of the immediate memory span for digits, an incident occurred which led the writer to believe that the memory span might furnish for the indication of complexes a technique comparable to the use of word associations and possibly supplementary to the word-association technique.

One of the subjects, a professional psychologist, displayed a peculiar difficulty with the number "2." Although his span for digit series not containing "2" was between eleven and twelve digits, he frequently failed on series with as few as nine digits if they contained a "2." His usual error was the substitution of "1" for "2," and this particular error accounted for approximately eighty per cent of all the errors he made.

When this circumstance was called to his attention, he stated he had not realized that he was consistently making the error, but that the "2's" seemed to "stand out" from the other numbers whenever they were read and to cause some mental confusion. He immediately suggested that the phenomenon might be the result of an operation he had undergone fourteen months previously in which a tumorous testicle had been removed. He stated that five days after the operation he had experienced a severe emotional depression lasting for two days in which he felt an unaccountable impulse to weep. Although he was facing a difficult real situation at the time, the depression did not seem to be consciously connected with that situation, nor was the impulse to weep a usual reaction to depressing circumstances. He had speculated upon its being a reaction to the castration trauma.

Prior to the operation, the subject had been tested for memory span for digits. At that time he had never had difficulty with series as low as nine, nor had the special disability with regard to the number "2" been noted.

In a later phase of the experiment, the memory span for word series was being tested. The series were composed of the names of domestic animals. One of the subjects, a Freshman student, was observed to make a consistent substitution of "dog" for "cat." He was unable to offer an explanation.

A priori arguments can be advanced in favor of using the memory-span technique for complex indication. With series that approach the maximum span of a subject, the closest attention is required, and the slightest disturbance may result in a breakdown of recall. Hence it might be assumed that the span would provide a delicate instrument for the detection of the inhibitory influence of a complex upon any of the words or numbers in a series. A further advantage is that, since the subject must react to the entire series at once, he is likely to be unaware of any difference in his reaction to the complex indicators. Finally, series with the complex indicators in them can be presented several times, without the subject's knowledge of his differential response, until the presence of a consistent difference can become a statistical certainty.

A test of this technique in clinical situations should prove interesting and possibly quite profitable.

REVIEWS

EDITED BY STANLEY G. ESTES

SOCIAL PSYCHOLOGY. By Charles Bird. New York: D. Appleton-Century, 1940. Pp. xiii+564.

SOCIAL PSYCHOLOGY. By Otto Klineberg. New York: Henry Holt, 1940. Pp. xii+570.

These two texts illustrate two divergent emphases in current social psychology. Bird's book is in the tradition of the experimental laboratory, with consequent centering upon highly civilized (better, highly mechanized) human beings in occidental culture. Klineberg's book draws heavily upon anthropology, with its non-experimental observation. Bird only once or twice mentions primitive tribes; Klineberg refers to 148 different primitive cultures. Klineberg has little discussion of scientific method and none of statistics. Bird gives details in many chapters of the techniques for using experimental and control groups, includes eight pages of technical "Essential statistical concepts," and writes an entire chapter on *methods* of measuring attitudes. The two books, thus, do not overlap so much as complement each other. Two students, however, would have vastly differing outlooks on social psychology as a science, depending upon which of these "Social Psychology" texts they read.

Almost every text in beginning psychology points out, usually in the first chapter, that science has three main aims: description (or understanding), prediction, and control. What do these two representatives of an extremely young branch of this young science have to offer in terms of these general aims of science?

Description. Both authors, having survived the battle of behaviorism, feel no necessity for justifying what introspective reports they include. Both, however, rely heavily upon objective behaviors of individuals.

Bird approaches operationally the area he is about to describe: "The field (of social psychology) is best defined in terms of what social psychologists are doing through researches and analyses" (4). Farther on: "A social situation cannot be defined apart from the behavior of individuals" (5). Finally, he says that social psychology is concerned with "the relationships arising as individuals interact with each other and with the stimulus objects constituting the social situation. These relationships are not dependent upon individual behavior alone . . . Group behavior gets its distinctive character from the situations created by interaction" (7). Throughout the book, Bird manages to use terms which refer to $Smith_1$, $Smith_2$, etc., doing things which react upon $Jones_1$, $Jones_2$, etc. There is no "custom" dictating actions: "The child is either directed or dominated by elders who insist that he meet their interpretation of social codes" (50). Again, "The principles of custom law are tacitly accepted; they issue out of the social experiences of large numbers of people; they are not to be confused with code law, which may be forced upon a person by a member of another group than his own" (358). Regarding delinquency, Bird makes it clear that "culture" does not teach, but rather: "What is learned must be acceptable to society broadly conceived or at least to the members of society having the power to dictate the rules" (454).

Klineberg, in his introductory chapter, says: "Social psychology may be defined as the scientific study of the behavior of individuals as related to other individuals . . . It would be difficult to point to any substantial amount of psychological description of the individual which does not reveal social influences at least to some degree." He broadens the area, in contrast to Bird, when he says he is "attempting to write a social psychology mainly from the standpoint of the relation of the individual to his culture, and to effect at least a partial synthesis of the science which deals with culture and the science which deals with the individual" (7–8). But throughout the book, Klineberg shifts from a description of individuals and their interrelations to a social and cultural determinism. "Culture," "mores," "society," "groups," "folkways" somehow seem at times to possess a power over individuals. For instance, anthropology has given an insight "into the manner in which culture may determine activities formerly regarded as instinctive" (12); "folkways may counteract this organic (sexual) drive" (121); incestuous relations are "restrained by the folkways of the group" (135); young Maori at present are losing the ability of their ancestors to weep at will, due to a changed education, which "is an example of the manner in which a change in cultural pattern may affect deep-seated behavior tendencies" (186); in connection with race prejudice, "aggressive tendencies (of mature people) are kept in check by the folkways of the group" (380); in the chapter on personality, it is held that the life-history technique is valuable because "it illustrates the dichotomy between a culture pattern and the experience of the individual" (499); in analyzing crime, "it is certain that the folkways of any particular group also contribute to the amount and the nature of the crimes committed" (536).

There are, undoubtedly, human activities and interactions behind these cultural entities Klineberg seems to use as causal agencies. Yet in the interests of accurate description, it would seem that he should stress them more. After all, the folkways are a convenient, though perhaps confusing and misleading, way of describing the *ways* of particular *folk*. "Culture," "mores," etc., are Smith, Jones, etc., doing things they want to do, or things other people want them to do. "Culture" does not make me take off my hat in an elevator; I take it off because someone told me I should.

Other differences in description are these. Bird has no discussion of the considerable work done in animal social psychology, while Klineberg has an entire chapter devoted to it, which includes such topics as: "The In-Group and the Out-Group"; "Animal 'Culture'"; "Language among Animals." The most striking omission in Bird's work is his almost complete neglect of the social psychology of language. Klineberg, by contrast, says: "It is hardly possible to overestimate the part played by language in the development and control of social behavior. It represents what is specifically human in social life . . . It is no exaggeration to say that language is one of the fundamental facts of human social life" (49–50). He consequently devotes an early chapter to questions of the origin of language, language and thought, developmental stages in the individual, etc.

Social psychologists have rather infrequently described the social factors involved in age. Bird devotes an unusually stimulating chapter to the social changes occurring at present in this country and abroad due to the increasing age of the population. Klineberg mentions this problem only in connection with aging among primitives.

It seems rather curious that in an industrial civilization such as ours, two

social psychologists should both neglect the human problems of factory production. The Hawthorne experiment of the General Electric Company, to mention only one study, resulted in both methods and findings which are of paramount importance to a scientific description of social life today.

Prediction. It seems to the present writer that there are at least three extremely important areas in modern life where predictions, even though little above chance, are urgently needed: the areas of war, dictatorship, and unemployment. A few important questions are the following: Will it ever be possible to eliminate war, or is there an instinct, prepotent drive, biological necessity for it? What are the chances that the remaining democracies will develop into dictatorships? If dictatorship develops in this country, will it be accompanied by persecution of minority groups? What reactions are apt to develop in a mature man who is out of work for a period of years? in a boy who has never found work?

Regarding a possible biological drive which inevitably results in war, both authors are in agreement. There is no such drive. Both devote considerable space to war; Bird, appropriately, ends his book with a chapter on it. He predicts that we will have war as long as conditions of peace lead to frustration and disappointment for many people. *"The point is not that men like war, but rather that men dislike many of the circumstances of peace"* (524, italics in book). Bird devotes little space to predictions involving dictatorship. Klineberg, while not specifically predicting, does devote space to the frustration-aggression hypothesis, and by implication predicts that chronic frustration will lead to non-democratic forms of government. Both authors give data on unemployment from which the student can make predictions.

A serious criticism of both authors is that they do not specifically make predictions, and tell what the chances are that each happening will occur. One test of a science is its ability to predict. If more social scientists would evaluate in terms of prediction, progress would certainly be more rapid, and trivial experiments would probably be fewer.

Control. Though there are many statements in Bird's text of the form that "if you want to get this result, do this and this," Klineberg has the more complete and specific summary. "If there is one general conclusion which may be said to emerge from this survey of the available knowledge in the field, it is that most of the characteristics as well as the institutions of modern society are to be explained in terms of social rather than biological factors . . . Criminality and mental abnormality may best be attacked by social betterment, and not by sterilization; wars and conflicts are not inherent in human nature, but in the structure of our social and economic system; racial hostilities are not due to the innate characteristics of biologically distinct groups, but to historical and economic forces . . . Man is primarily a social animal, and his hope for the future lies in the improvement of his society" (549).

Which text will interest students more? Without hesitation the reviewer recommends Klineberg's. The material in his book is more vivid, there are more striking examples of social behavior from primitives, there is more material on the relation between the sexes than in Bird's work. The latter, with its emphasis on methodology, will appeal to students intending to specialize in psychology. Together, they make an excellent introduction to social psychology, vintage 1940.

ARNOLD THOMSEN.

Syracuse University.

PERSONALITY STRUCTURE IN SCHIZOPHRENIA: A RORSCHACH INVESTIGATION IN 81 PATIENTS AND 64 CONTROLS. By S. J. Beck. *Nervous and Mental Disease Monograph,* No. 63. New York: Nervous and Mental Disease Monographs, 1938. Pp. ix+88.

The author assigns a two-fold purpose to this work: the determination of the value of the Rorschach as a diagnostic device for schizophrenia, and an analysis of the psychological meaning of the findings for the personality structure of this group. The subjects comprised a group of 81 schizophrenics from the Boston Psychopathic Hospital and 64 controls, 33 of whom were normal (mostly connected with the hospital in some capacity) and 31 "non-psychotic" (persons who had been studied psychiatrically but whose examinations did not yield evidence of any psychotic trend, damage to neural tissue, mental deficiency, or psychopathic personality). Although the samples were fairly well matched for age, the education of the control group was distinctly higher, over twice as many falling at or above the superior level as in the patient group.

The analysis is introduced by two well-matched cases fairly typical for the normal and the schizophrenic respectively. The Rorschach responses, scoring, and summary are given in detail for each. To answer the question as to how regularly the differences found in these subjects occur in representative samples of the two groups reported, a statistical analysis of the results obtained is presented. The major statistically significant differences found were as follows: greater sensitivity on the part of schizophrenics to rarer details, a greater tendency to interpret the whole figure on the basis of suggestion from a detail, responses more frequently determined by color, forms much less accurately perceived, and most common responses less frequently given. Contrary to expectation, movement responses are not different in frequency from those found in normal subjects.

The psychological implications of these Rorschach findings are analyzed individually. The "average" picture obtained of the schizophrenic is that of a person with a greater sensitivity to minor details, with poorly distributed attention, and with lesser orderliness of intellectual procedure. In the affective sphere there is evidence for his being more likely to manifest impulsive, unstable behavior and for being in lesser rapport with the environment. On a more intellectual level such evidence is corroborated by his definite inferiority in the accurate interpretation of presented material and his lesser commonality of response. In order to correct any false implications which may be drawn from this "statistical" schizophrenic, the author devotes a chapter to the interpretation of the data obtained from the schizophrenic patient previously presented.

On the basis of this concrete material the author develops a theory of personality. There are, he holds, two aspects to personality, the psychological and the social; a full understanding of a person can be achieved only from a study of both of these, together with the balance (organization) among the psychological characteristics. In the normal person, according to Rorschach standards, this balance would be expressed in the presence of proportionate amounts of F+ (seeing the world accurately), Z (grasping larger relationships), M (richness in imagination) and CΣ (affect, drive). In schizophrenia, although the psychic structure indicates a grasp of relationship (Z) and imaginative living (M) not much different from the normal, the balance is disturbed in that appreciation of reality (F+) is much reduced and affect (CΣ) is significantly increased. Instead of there being a greater appreciation of reality in order to

compensate for the increased affect, the opposite is the case. With respect to the social personality the Rorschach can give only a partial answer, since this information comes from the nature of the particular content of the responses, and these differ from patient to patient.

In a last chapter the author considers certain miscellaneous signs manifested by the schizophrenic which occur too infrequently for quantitative treatment or are qualitative in nature. Some of these are position responses, reversal of figure and ground, change of tempo, perseveration, and contaminations. A bibliographical note completes the presentation.

The author continues in this volume his pioneer work in the elucidation and application of the Rorschach test. The style is lively and conversational—a tendency perhaps pushed occasionally a trifle too far. This reviewer finds it difficult to become accustomed to the use of "healthy" for the more generally accepted "normal"; somehow a picture of an Iowa 4-H contest winner persists in obtruding itself at each such mention! Too, the reference to the Rorschach "experiment" (more persistently used in others of the author's works) gives the impression of a degree of control which the test procedures do not ordinarily have. Although the distinction between test and experimental situations cannot clearly be drawn, the method of administration used in the present study obviously falls into the former category. These are, however, minor points. More important is the fact that the author has not taken his own recognition of the danger of lumping schizophrenics together (p. 14) seriously. An essential methodological point is involved here. Paranoids and hebephrenics (to take the most clearly distinguishable types and to accept a classification system which admittedly has its weaknesses) have in a number of studies been shown to fall on either side of the normal in certain characteristics. The combining of groups tends to wipe out important differences by pushing towards unimodality tendencies which actually go in the direction of bimodality. It is likely that a number of the negative results obtained in dealing with schizophrenia may be due to such disregard of fundamental syndromes.

The personality structure of schizophrenia which the author develops is in considerable harmony with clinical, test, and experimental findings as well as with the work of other Rorschach studies such as those of Skalweit, Rickers-Ovsiankina, and Rorschach himself.

The monograph will undoubtedly take its place as one of the important items in the Rorschach bibliography of schizophrenia and is, of course, essential to the Rorschacher. One must, however, admit that the reader lays down the volume with a feeling of incomplete satisfaction—due perhaps to the fact that acquaintance with the extent of the experience and background of the author in psychopathology had led one to expect a much more detailed and extensive consideration of the subject than is actually presented. Now that the statistical aspects have been considered, cannot we expect from him a supplementary essay relating clinical schizophrenic pictures in detail to Rorschach findings?

D. SHAKOW.

Worcester State Hospital.

THE VARIETIES OF HUMAN PHYSIQUE: AN INTRODUCTION TO CONSTITUTIONAL PSYCHOLOGY. By W. H. Sheldon with the collaboration of S. S. Stevens and W. B. Tucker. New York: Harper, 1940. Pp. xii+347.

The preface of this volume begins like a syllogism: "In the book *Psychology and the Promethean Will* (Harpers, 1936) the optimistic proposition was put

forward that psychology may in the course of time prove useful. It was suggested that to be useful, psychology needs first of all to bring system and order into the description of basic differences among human beings. From such a point of view the first task of psychology seems to be that of standardizing a method for describing quantitatively the varying physical endowments of individuals."

This quotation provides a keynote for the entire study. The proposition that psychology may in the course of time prove useful is followed up by plentiful illustrations of guidance and adjustment problems for which the psychology of today does not yet provide answers. The need for system and order is insisted upon in every chapter. And the investigations which are reported in detail result in a method for classifying physiques through the use of standardized photographs which has exceptionally high objectivity, i.e. a correlation of .95 between the independent appraisals of two trained experimenters. This reviewer would question, however, whether the formalized plan of presentation has not been achieved at a certain cost.

To suggest that psychology may come of age and eventually be of some use is an audacious and amusing bit of rhetoric. But it may be more than mere coincidence that the author of such an insurgent figure shows a tendency to overlook modern work and viewpoints that are relevant to his own undertaking, and to assume paternity for ideas which already have a vigorous life of their own. Thus (p. 226) he decries the "usual publications of height-weight-age norms" which take no account of constitutional body build and give "some of our best people inferiority complexes." But in proposing a counteractive in terms of height-weight standards for his different somatypes he fails to note the important advances that have already been made in New York, Iowa, California, and elsewhere on predicting normal weight from multiple body measurements. When he states that his point of view in its emphasis upon constitutional differences and its advocacy of differential treatment early in life "is counter to the current educational philosophy which stresses the virtue of socializing and leveling influences—forces which protect the child from differential development" (p. 227), he is challenging the entire Progressive Education Association to inquire, *whose* current educational philosophy! He is also inviting an acid "You're telling us!" from eugenicists who for many years have been urging a "positive eugenics," when he writes with an air of fresh discovery (p. 229): "For a long time now there has been talk of a eugenics program, but it seems possible that we have hitherto attacked the problem from the wrong end. We have talked only in terms of suppressing the unfit and of eliminating or sterilizing a fringe of unfortunates. . . . May it not be more desirable to tackle the problems at the other end and to discover what parentage produces the best children?"

Such illustrations could be augmented, but would lead us afield from the other propositions in Sheldon's syllogism. Consider the second premise: to be useful, psychology needs first of all to bring system and order into the description of basic differences among human beings. And then the third term, or conclusion: the first task of psychology is the standardizing of a method for appraising physique. A logician would probably halt at the conclusion, even granted the premises, and many psychologists would probably object to the whole rationale as *a priori*.

If, however, we can disengage ourselves from the author's rather vulnerable blueprint construct and examine the data actually reported, we find an arresting

idea worked out with scrupulous technical care. Instead of classifying body build into merely three types (plus a dysplastic group showing mixed characters) as Kretschmer and many of his predecessors had done, Sheldon has used the three main morphological classes as graded components of physique, assigning to every individual who is "somatyped" a score on each of the three components gauged on a 7-point scale. Thus a person labelled 631 scores high on the first or "endomorphic" component characterized by soft roundness and massive digestive viscera, intermediate on the second or "mesomorphic" component determined by prominence of muscle, bone, and connective tissue, and as low as possible on the third or "ectomorphic" component for linearity and fragility. In a sample of 4000 young adult males, Sheldon was able to describe and illustrate 76 different somatypes represented by combinations of these three "primary components." Variations due to "secondary components" were also superimposed, i.e. "gynandromorphy" or bisexuality of physique, "texture," "hirsutism," and "dysplasia" or variation of type in different regions of the body. It is evident—and moreover the correlation scattergrams show clearly—that not all the theoretical combinations of the seven grades of the three components are physically possible to obtain, since an individual cannot rate extremely high on more than one variable, nor extremely low on more than two. For example no physiques 771, nor 111, nor 112, etc., occurred. Actually all physiques in the series of 4000 cases "summed" up to values between 9 and 12 when the ratings for the three primary components were added.

We have mentioned that the somatypes, which are assigned through an ingenious procedure which combines separate estimates based on measures from five different regions of the body, yield a highly objective classification scheme as judged by the correlations between appraisals of experienced raters. "Upon this foundation," the author writes (p. 264), "it may be possible to build the superstructure of a science of human behavior."

Here again, the reviewer is impelled to question the theoretical framework. While the method is objective, i.e. the results can be repeated *ad lib*, and the *extreme* somatypes show distinctive patterning, it is not clear that the three components are "primary" in the sense of being entities, nor that, for example, "endomorphy 4" achieved through rating 5 in the region of head and neck and rating 3 in the region of legs and feet would have the same somatypic significance as the same rating obtained through reciprocal designations. Neither is it clear that a rating assigned even in the same region always has the same significance. The head-and-neck region, for example, is rated on the basis of four photographic diameters, and it would seem possible that appreciable "dysplasia" could occur between these diameters. Until we have some way of identifying meaningful, coherent somatypes expressed through somewhat variable syndromes, as we can now do in the case of certain pathological entities such as Mongolism, we may view any scheme of classification as still on a research level rather than on a "foundation" level.

Perhaps the best possible way of testing such a classification as Sheldon's for its basic value is through just such relational studies of physique and temperament as are outlined by him for a future volume. In Chapter VII, which gives a condensed but highly readable prospectus of work in progress on constitutional psychology, he indicates a parallel classification of temperaments which "correlate with the components of physique, but not in any one-to-one fashion." These are, in brief, the "comfortable" *viscerotonic*, the active, energetic *somatotonic*, and the inhibited *cerebrotonic*, and are discussed by Sheldon in connection

with such diverse problems as mental disorder, differential needs in food, clothing and shelter, sexuality and mating, crime and delinquency, early education.

If this preliminary volume evokes an attitude of suspended judgment, the promised second volume stimulates a friendly anticipation.

BARBARA S. BURKS.

Carnegie Institution of Washington
Cold Spring Harbor, L. I.

THE INVASION FROM MARS: A STUDY IN THE PSYCHOLOGY OF PANIC. By Hadley Cantril with the assistance of Hazel Gaudet and Herta Herzog. Princeton: Princeton Univ. Press, 1940. Pp. xv+228.

Probably no field of social psychology has thus far been so inadequately studied as that of panics. Such a state of affairs is, of course, not the fault of the social scientist for, up to date, he has had few opportunities to make first- or even second-hand observations of these most interesting phenomena. Hence it was a lucky accident when Professor Cantril, who was already studying the socio-psychological effects of the radio, suddenly found, in his own back dooryard, an honest-to-goodness panic, and a radio panic at that. It was an adaptation of H. G. Wells' "War of the worlds"—broadcast on October 30, 1938, and mistaken for a series of news bulletins—which gives the data for this study.

Of course the matter could have been immediately settled with the aid of some smug, particularistic explanation. Thus, Dorothy Thompson dismissed the affair by blaming it on the "incredible stupidity" of the victims. A prominent but unnamed psychologist ventured the statement that no intelligent person could have been taken in. Another psychologist claimed that the disturbed persons were all neurotic. An unnamed sociologist thought that "only unintelligent people were listening to this anyway and that the chances were slight any others would be disturbed." Cantril is to be congratulated on avoiding such easy "explanations" and on attempting to uncover at least a few of the more pertinent of the factors which were involved.

That there was a panic seems not to be doubted. Whatever one thinks of the validity of the polling devices employed (A.I.P.O. and C.B.S. cooperated), he must agree that several (perhaps as many as six) million heard the broadcast and that at least a million were excited by it. Added data on the extent of the panic were obtained through letters, newspaper clippings, and through communications from the telephone companies. Careful case studies were made of 135 individuals; six sample stories, taken from these, are given in the book.

On the basis of these studies Cantril concludes that "a panic occurs when some highly cherished, rather commonly accepted value is threatened and when no certain elimination of the threat is in sight. The individual feels that he will be ruined, physically, financially or socially." M. Sherif's concept of the "frame of reference" is taken as a descriptive vehicle.

Cantril has classified his variables into three categories. The first is "the capacity to evaluate the stimulus in such a way that they (the at first frightened listeners) were able to understand its inherent characteristics so they could judge and act appropriately." This he has termed critical ability. It appears to be most closely tied to educational level. The two other variables prominent in the picture are the actual listening situation and the listener's personality.

It is highly probable that few social psychologists will have anything but

praise for the careful work Cantril has so far done on the study of panic behavior. The matters of critical capacity and the listening situation seem well handled. The major difficulty will be found to lie not in errors of commission but rather in those of omission. In the tests of personality the issue shows itself most clearly, for here Cantril follows too closely for the comfort of some psychologists the technique of "consciously reconstructing the intuitive process." Thus, personality susceptibility to panic is divided into seven categories—insecurity, phobias, amount of worry, lack of self-confidence, fatalism, religiosity and frequence of church attendance. Extremely rough measures of each are obtained. No test of intelligence is employed, as "experience of everyday life as well as psychological studies should convince us that even such an apparently uniform capacity as 'intelligence' has personalistic dimensions completely neglected when any given person takes a test that has been standardized for a large population." Tests of expressive, attitudinal and temperamental traits are ruled out for like theoretical considerations. To scientists of different theoretic bias these omissions will appear enormous.

But, regardless of the validity of these objections, the results of this study of panic behaviors certainly more than justify the relatively small amount of money spent. All social psychologists should be pleased to see a stubborn field at last opened through scientific attack.

PAUL R. FARNSWORTH.

Stanford University.

CONDITIONING AND LEARNING. By Ernest R. Hilgard and Donald G. Marquis. New York: D. Appleton-Century, 1940. Pp. 429.

"Theories of learning which make use of conditioning principles are not related in a simple manner to the facts of conditioning experiments. This book represents an effort to place the facts and theories into some sort of order through critical exposition" (p. v, Preface). The authors have accomplished this task exceedingly well. An exhaustive collection of data is assembled, and the exposition is effected with great clarity. Its appearance at a time when the literature on conditioning had begun to seem so voluminous that it almost threatened to overwhelm such an attempt at organization, makes its particularly valuable. The book is avowedly eclectic, and consequently suffers to a certain extent from the indecisiveness which is the almost inevitable concomitant of such eclecticism. Such a liability is quite outweighed, however, by its excellence as a reference book. The extensive bibliography (973 titles) will prove invaluable to anyone who works in this field, and the indexing, an important asset in such a book, is very good. The book is particularly to be recommended for its clarity, its completeness, and its non-partisan character.

The opening chapter sketches an historical background for conditioning and suggests some of the forces which may have been responsible for the grip which it acquired on American psychology. The next six chapters consist of a discussion of the characteristics of the conditioned response, presenting the relevant data. The content of these chapters is largely factual and explanatory, presenting experimental evidence relating to different types of conditioning. Conditioning is considered in terms of a stimulus substitute theory, of Thorndike's Law of Effect, and Tolman's Sign-gestalt view, and an attempt is made to show how each of these positions would deal with the diverse facts of conditioning. With this exception the section is devoted almost entirely to reporting the data

and stating the problems, and as such it affords a good introduction to the problems of conditioning.

The relation of conditioning to discrimination, serial learning, problem-solving, and voluntary action is taken up in the next four chapters. These chapters, which are not at all polemical, are marked on the whole by an impartial statement of both the Gestalt and associationistic view of these problems, a condition which has not always applied in the journal reports of the material. The associationist's perspective comes most readily to the authors, however, and in considering these questions, where the conditioned response has been most vigorously challenged as an explanatory principle, the view of the field theorist seems to be slighted. Lewin and Tolman play the major roles in forming Gestalt theory as it is presented here, and many interesting and challenging points (e.g., the work of Köhler, Adams, Honsik and Tolman, Helson, et al.) are given inadequate treatment, or are relegated to the notes at the end of the chapters. The important and relevant problems of perceptual organization, the structure of the behavioral environment, and the definition of the stimulus are not sufficiently elaborated. The final "appraisal of conditioning as an explanation of learning," in spite of the authors' reluctance to adopt a systematic position, leans heavily on Professor Hull's views, and seems to imply a rosy prognosis for such an explanation.

The final chapter concerns the neurophysiological mechanisms of conditioning; here the discussion loses some of its force. Not only is the whole presentation oversimplified, but the emphasis given to Pavlov's view of irradiation and to an "altered pathway" explanation of conditioning does not harmonize well with the complex problems which were taken up before. Although Pavlov's theory of irradiation is discarded as too simple, no hypotheses are offered concerning a cortical process underlying the irradiation inferred by Hull, Spence, Hovland, et al., to account for generalization, while the possibility of a field theory (in physical terms) of cortical activity is summarily dismissed as implying "action at a distance." The final verdict on the locus of conditioning is that it must be an alteration of the characteristics of one of the specific pathways involved, and the views of Lorente de Nó are given the most extended treatment in discussing this question. Unfortunately the authors did not take this opportunity to bring together the facts and possibilities of chemical mediation which have an interesting relevance to this problem.

The appearance of a chapter on personality is a notable achievement in a book on learning, and will perhaps facilitate a better rapport between the two fields. Discussions of personality must necessarily lean heavily on a theory of learning in accounting for the facts of development, and in too many cases the theorist in personality has relied on a view of learning which has been largely outmoded. This chapter, unfortunately, deals almost exclusively with the familiar "animal neurosis" experiments and their implications for abnormal behavior. In spite of the fact, however, that there is no explicit discussion of the relation of the problems of learning which precede this chapter to those of a theory of normal development, it seems clear that this is a vital point in a complete picture of personality. The arguments against the CR as the unit of habit, and the deviation of Hull's position from this view are perhaps examples which are relevant in this connection. The discussion of animal neuroses and the relation of conditioning facts and theories to some aspects of Freudian theory is particularly well handled.

This book will serve admirably to pave the way toward a complete theoretical

formulation of the problems of conditioning and learning, and any theory which can adequately encompass the variety of facts which are set down here will have accomplished a great deal.

MASON HAIRE.

Harvard University.

EMOTION AND CONDUCT IN ADOLESCENCE. By Caroline B. Zachry. New York: D. Appleton-Century, 1940. Pp. 544.

The author describes this volume as "the first book prepared for the Commission that is devoted wholly to the observations of the study and the only one that attempts to discuss these in the full scope of their implications for secondary education." The author further states in the preface that "the various techniques of observation that were employed . . . yielded insights into the educational needs of students," and that "it is from this concept of needs in adolescence . . . that the observations regarding adolescent development are formulated." Thus the reader is given fair warning that the book is not going to be a report of research such as Howard Bell's *Youth tell their story* (a publication of the American Youth Commission) or Dimock's *Rediscovering the adolescent*. Each of these earlier books is devoted to a discussion of the problems unearthed by specific research—the first sociological, the second a cumulative study of the same group of adolescents over a period of years.

The present book is specifically intended for "high school and college teachers and guidance workers and other specialists concerned with secondary education." The discussion covers a full complement of topics pertaining to the adjustments of adolescents under the three main headings of "Changing attitudes to the self," "Changing personal relationships," and "Changing attitudes to basic social institutions."

The psychological ramifications of each of these general titles are extensively treated. Under the heading, "Changing body and changing self," the interest is centered not upon what physical changes take place in adolescence (though this aspect is presented in a general discussion by Benjamin Spock, M.D.) but upon the implications for personality development of these changes. How the adolescent reacts to his physical appearance is highly important, as Zachry points out, and how he compares with his classmates in physical maturity enters into his evaluation of himself. Likewise, suggestions are given as to how cultural concepts and family attitudes toward the differential role of men and women may color the adolescent's picture of himself.

From a consideration of the factors which contribute to an adolescent's attitude toward his physical development, the author goes on to discuss in like manner the adolescent's task of adjustment to standards of conduct. Some of the mechanisms of compensation are considered here. Finally she discusses in this first part the role of education in meeting the needs which arise in the adolescent as he adapts to his changing attitude toward himself.

In Part II, "Changing personal relationships," the first section considers the subject in broad outline including "The adolescent's task," "Social trends influencing development in attitudes toward others," "The child's developing attitudes to protection and authority," and "The desire for emancipation." These topics are further elaborated in the following two sections more specifically in

dealing with the adolescent's relationships with adults and with peers. Educational implications conclude Part II.

The third part of the book, "Changing attitudes to basic social institutions," covers the subjects of "Vocations," "Citizenship and marriage," and "Education and the approach to adulthood."

The reader comes to think of the book as a compendium of treatises on adolescent adjustment with precepts for education written from the point of view of a clinically minded person who has had extensive experience with the progressive education movement and with adolescents in school, and who is mindful of the present American scene. Throughout there is emphasis upon the difficulty and complexity of the task facing adolescents.

One precept underlying all others is this: "Recognition of the presence and probable import of unconscious motivations in conduct is necessary to understanding of personality development, although concern with the dynamics of these processes specifically, in any given individual student, is not usually in the province of the educator."

There is a disturbing number (over one-half) of youngsters referred to in the illustrative material whose family relationships, especially those with the mother, are contributing if not major causes of difficulty. If these cases are a representative sample, perhaps there should be more extensive consideration of the implications for parent education. Of the fifty-five sketches, only four indicate that the parents' attitudes toward their children were helpful.

An encompassing work of this sort is perhaps inevitably uneven in quality. There are some topics which the author handles with unusual ability, presenting the adolescent's problems convincingly and suggesting helpful methods for an effective educational approach.

The writer's presentation is so emphatically in general terms of adolescent needs and educational implications that of the more than five hundred pages of text, illustrative material from the study covers not more than fifty. Most of this, moreover, is not in the form of original data, but is material reworked into brief case histories or personality sketches. No figures, no charts, and no tables are included in the book. In other words, the reader is asked to take it for granted that insights were obtained which led to educational implications, and not to expect to be able to discover which data led to which insights or which insights in turn led to which educational implications.

For example, in discussing the statement that physical and psychological factors are interrelated in behavior, the author writes, "Of one group included in the study, the medical observer made the general comment that there was a not inconsiderable degree of correlation between physical fitness and personality adjustment as estimated by his colleagues in the field of psychology."

Since in this instance and throughout the book the author deals with the subtleties of interrelationships—variables which cannot be accurately measured—we cannot criticize her for the omission of correlations. Yet to the reader who likes to refer to data for the evidence, statements as general as the above are unsatisfying. Perhaps the author believes that she is performing a greater service to education in presenting an "overview" of impressions from her research program than could be rendered by limiting herself to discussion of data.

One pitfall in such an approach comes from the tendency which it engenders to cover too much territory. The title suggests that this book may have fallen into such an error. In its scope it suggests *Democracy and the curriculum*, a

recent book to which Zachry was one of ten contributors. Such a compilation is enlivened by the fact that it presents a variety of literary style and several points of view.

MARY COVER JONES.

Institute of Child Welfare
University of California

A VISUAL MOTOR GESTALT TEST AND ITS CLINICAL USE. By Lauretta Bender. New York: American Orthopsychiatric Association, 1938. Pp. ix+173.

Lauretta Bender's Visual Motor Gestalt Test consists of nine drawings, which are presented to the subject with the instruction to copy. These drawings are taken from a paper of the founder of the Gestalt school, M. Wertheimer, and so far as the reviewer can see this is the only reason why the test is referred to as Gestalt Test.

The book itself is divided into theoretical and clinical sections. The former reports on: (1) the administration of the test to young children (3–7 years old), as well as the observations of the spontaneous play productions of children; (2) the reactions to the test by low-grade adult defectives, whose motor patterns are, however, mature; (3) the analysis of A. W. Nissen's data on the drawings for the Army Performance Test as given to 50 native African children (5–13 years old). From these investigations the conclusion is reached that "Gestalt arises from a state of flux in the sensory field and it is always in a state of flux. . . . The development of spatial organizations follows a definite pattern in the different maturation levels."

The theoretical section further contains reports of experiments with: (4) imagining the Gestalt figures; and (5) their reproduction after tachistoscopic exposure. The findings serve to confirm once more the above "theory" and to supplement it by emphasizing the importance of the time factor in the "Gestaltung" of the experience. If the time of exposure is insufficient, the pattern experienced by an adult is similar to that observed by a small child.

Numerous reproductions of drawings offer very interesting material to those readers who are not familiar with these phenomena. Essentially they are not new, however, the novelty lying chiefly in very sweeping statements and theoretical generalizations made by the author. The theory expressed is certainly not a theory in the sense that it leads to predictions and conclusions or even factual statements of a specific nature. What is important to know is not that these influences exist but exactly in what way they are responsible for a particular perceptive phenomenon. The author forgets that psychologists themselves are very conscious of the fact that they have not yet mastered certain dynamic features of perception as well as of other fields of psychology. More is needed, however, than just pointing this out.

The clinical part of the book presents a contribution of greater value than the theoretical. One regrets only that so little use is made of statistics. Except for the chapter dealing with the standardization of the test on children, one never knows how frequent and consistent the described patterns of behavior are in the various clinical groups.

The test gives an estimate of mental age in children and defectives in a way similar to the Goodenough test. The present test, however, since it employs meaningless figures, has the advantage that the picture of intelligence is not obscured by language and training. For the same reason it is valuable in follow-

ing up the progress of transient states of mental disturbances, e.g. aphasia. With respect to malingering the study demonstrates the use of such drawing tests, when well standardized. In schizophrenia, manic-depressive psychosis, and psychoneurosis the subjects apparently behaved as one would expect a patient of that type to behave.

On the whole, despite the criticisms which one might make especially of the theoretical section, the study is a stimulating one and the test has considerable promise for clinical purposes.

M. A. RICKERS-OVSIANKINA.

Wheaton College.

MYTHOLOGY OF THE SOUL: A RESEARCH INTO THE UNCONSCIOUS FROM SCHIZOPHRENIC DREAMS AND DRAWINGS. By H. G. Baynes. Baltimore: Williams and Wilkins, 1940. Pp. 939.

In this exhaustive study of two borderline schizophrenic patients, both men, the origins of the psychotic split and the process by which it was healed, are presented to the reader in graphic form. In his discussion here, though probably to a far less extent in the consulting room, Dr. Baynes carries on the analysis of his two cases by means of drawings which the patients themselves made while under his care.

Dr. Baynes is an analytical psychologist of the Zürich School. He was a personal pupil of Dr. C. G. Jung and later his assistant, so that he is unusually well equipped to expound the synthetic method of depth analysis introduced by Dr. Jung.

Many of the pictures reproduced here (there are 48 drawings by his two patients, besides 10 other figures illustrating the text) are definitely psychotic in character, with typical distortions, "broken lines" and confusion. Yet, as Dr. Baynes deals with them, they begin to show meaning and to reveal a "myth" working itself out entirely beneath the threshold of the patient's consciousness. Not only are the individual pictures analyzed in the text, but they are also dealt with in series, where the various symbols and formations are found rearranging themselves and being transformed, so that gradually the separated parts begin to come together and form a new and meaningful organization.

Interspersed with the clinical material—the pictures and dreams and related associations of the patients—Dr. Baynes has given us short summaries of corresponding mythological material, which not only parallels the transformation he is describing, but also serves to illuminate the whole process. Indeed, without a very full knowledge of anthropology and of religious texts and symbolic forms, it would hardly have been possible for him to have unravelled the seemingly chaotic material with which these two patients presented him.

The whole book is written in an easy and flowing style; Dr. Baynes is certainly to be congratulated on the skill with which he has made most difficult and complicated material accessible in an understandable and interesting form. The work on the picture and dream material is far more detailed and consecutive than is usual in studies in this field and in consequence should prove exceedingly stimulating and suggestive for the practicing analyst, as well as informative for those who are interested in the problems which are dealt with, even though they are not specialists in the field of depth psychology.

M. ESTHER HARDING.

New York City.

THE BIOLOGY OF HUMAN CONFLICT. By Trigant Burrow. New York: Macmillan, 1937. Pp. xi+435.

THE PSYCHOLOGY OF HUMAN CONFLICT. By E. R. Guthrie. New York: Harper, 1938. Pp. ix+408.

FULCRA OF CONFLICT. By Douglas Spencer. Yonkers-on-Hudson: World Book Co., 1939. Pp. xii+306.

Dr. Burrow's main thesis in *The biology of human conflict* is that man, through participation in a "neurotic" social order, has been forced to adopt a "social image" or mask which renders him incapable of making unitary and full responses to the real biological environment. The partial responses which ensue are in conflict with each other as well as with the basic motivational substrate which marks man as a phylum. The author discusses the implications of this process in relation to the group analysis which he and his co-workers at the Lifwynn Foundation have been conducting, the aim of which is to reinstate in full measure the unitary and total nature of response. Whether this end is achieved by instituting a "non-neurotic" environment or through some other means is not made clear, nor are the results of the work stated in a straightforward expository manner. The author's emphasis on conflict as a physiological state and the consequent necessity of physiological therapeutic measures, though challenging, seems only loosely related to the "phyloanalytic" or group-analysis work being conducted at the Lifwynn Foundation (unless the word "physiological" is being used in an unusually unorthodox fashion). The reader is likely, in addition, to find the complexity and novelty of the terminology confusing.

Professor Guthrie's engaging book represents an attempt to reduce the problems of conflict and its attendant manifestations to the author's *genre* of "stimulus-substitute" conditioning theory. Conflict, conceived as the clash of antagonistic response tendencies, is analyzed in terms of the conventional conditioning concepts of reinforcement, inhibition, facilitation, etc. In his analysis of the mental disorders resulting from conflict, Guthrie leans heavily on Janet's clinical descriptions, conceptual viewpoint, and therapeutic system. The occasional translations of Janetesque clinical pictures into the language of conditioning theory often results in speciously clear and simple "explanations." In addition, the writer's brilliant use of analogy and anecdote, though stylistically charming, robs the book of the kind of clinical concreteness so badly needed in this field. This volume, being an extension of the author's theory of learning, makes an excellent companion to his *Psychology of learning*.

Fulcra of conflict deals with the construction of a battery of tests for adolescents designed to reveal those areas of behavior in which ground for conflict is present. The battery consists of seven tests: Section A, "Your own beliefs, ideals, and aspirations"; Section B, "Your mother's beliefs, etc."; Section C, "Your father's beliefs, etc."; Section D, "A description of your mother"; Section E, "A description of your father"; Section F, "A description of your closest associates"; Section G, "Your best judgment about yourself." Each of these sections covers eight overlapping areas in which conflict might occur. These are social, home, and school life; personal achievement and problems of personal characteristics; health and moral conduct; and economic problems. By the use of corresponding questions in each section, a measure of the divergence of the testee's best estimate of himself (Section G) from his estimate of his own and parents' ideals and his parents' and friends' behavior can be determined.

An experimental form of the test-battery has been administered and, although no direct validation was attempted, presumptive evidence of validity in the form of a high measure of internal consistency is presented. High correlations, however, between section scores and total battery scores and between individual sections are very questionable indices of validity when applied to this type of material. Divergence of one's own ideals from those of one's father need not indicate, for example, a high general conflict score nor a high conflict score on any other section of the battery. The conventional "consistency" technic of validation is simply not applicable here. Since the author reports that he is attempting a validation by the use of the interview, these questionable findings can be temporarily overlooked. Work on the test is continuing, and the ingenious beginning reported in this book gives promise for the development of a useful instrument for the study of conflict. The reviewer, however, is at a loss to understand why, with the research still in such an unfinished state, the author undertook publication in book form.

JEROME S. BRUNER.

Harvard University.

CAUSES OF CRIME: BIOLOGICAL THEORIES IN THE UNITED STATES, *1800–1915*. By Arthur E. Fink. Philadelphia: Univ. of Pennsylvania Press, 1938. Pp. x+309.

THE SEX CRIMINAL. By Bertram Pollens. New York: Macauley, 1938. Pp. viii+211.

CRIME AND THE COMMUNITY. By Frank Tannenbaum. New York: Ginn, 1938. Pp. xiv+487.

In his *Causes of crime* Dr. Fink brings together in a clear and straightforward manner the various doctrines developed in this country from 1800–1915 that seek to account for the cause of crime. He is not interested in a discussion of cause as such; he carefully avoids this. Nor does he discuss the environmental background of crime. He has the historian's zeal to show the development of ideas throughout the period. He accomplishes his purpose admirably with a story that takes us from the period when the madman and the criminal were indistinguishable down to 1915 when William Healy's *Individual delinquent* marked a new interest in the psychological and sociological study of the offender as a personality. Theories of crime causation based on studies of phrenology, insanity, moral insanity, alcohol and drugs, criminal anthropology, heredity and feeblemindedness, are systematically reviewed and evaluated. It is evident that the physician in the pre-Healy days was as much interested in the subject as the physician-psychiatrist of the present time. It is interesting to observe with what confidence their views were propounded—views that are today considered totally inadequate as explanations of criminal behavior. Criminals were at times thought to be quite different in kind from ordinary people, as though they belonged to a different species of humanity, and throughout this whole period there was a strong belief in the inheritance of crime. The learned physicians, we now believe, were in error, but "it was the error they shared with their generation" and the author cautions us against evaluations not made "in the light of the knowledge and experience of their day." All too frequently we assume the light and experience of our own day as final simply because they are contemporaneous. This first systematic presentation of American thought

in the field is a valuable piece of research, well-documented and containing some 775 references.

The second book, *The sex criminal,* deals also with causation but confines itself to a special form of criminal behavior that the author believes can be accounted for in terms of a single theory.

The sex offender stirs the deeper emotional currents of the man in the street. The popular clamor for retribution is reflected in the law's reluctance to provide adequate psychiatric study or to send the offender to an institution where treatment rather than punishment would be the rule. The dangerous psychopath is punished for a time, then released, just as any ordinary criminal would be. It seems to the reviewer that those who maintain the rigid structure of the law are not eager to relax in favor of the sex offender by relieving him of responsibility and the punitive consequences of his acts; in fact, the law-maker's propensity for vindictiveness is all the greater toward one who dares to threaten his own emotional balance and control.

In contrast to this spirit of righteous indignation, the senior psychologist of the sex clinic of the Penitentiary of the City of New York, presents the case for a program based on "objective, scientific data." Taking cases from his clinical practice, he seeks for a systematic explanation for the behavior of the sex offender in terms of Freudian theory, giving the reader a brief account of the psychosexual development of man, and showing how the final anti-social sex act can be accounted for. "Scientific procedures" are suggested, and radical changes in the law are recommended in accordance with the "modern, scientific knowledge" at the disposal of the Freudians. One looks in vain, however, for the application of the scientific method in the clinic, for the citation of experimental findings for any "objective data" in the book. Statements like the following, so frequently found in popularizations of Freud—generalizations without evidence —are not the kind to convince the practical mind of the law-maker that he should turn to the psychiatrist for guidance: "Scientists who specialize in microscopic work have sublimated their peeping instinct, while physicians who derive pleasure from the practice of surgery have degenitalized their sadistic trends." The book points out that the layman has little understanding of the problem. The reviewer wonders if a brief and popular presentation, reciting details of many a sensational sex crime, will add to his understanding.

Professor Tannenbaum, in his *Crime and the community,* taking a very practical view of the present situation, covers the broad field of criminology and penology with an experienced eye. Of greatest interest to psychologists is his attack upon all who would explain crime in terms of the physiological or psychological abnormalities of the individual offender. Current theories that attempt to show qualitative differences between the criminal and the non-criminal are, he argues, no more valid than the discarded theories of the last century. Psychologists and psychiatrists simply substitute the words "normal" and "abnormal" for the time-worn "good" and "evil" with a pretentiousness that stirs him to a vigorous denunciation. He would not say that the criminal is a maladjusted individual; he is, in fact, well-adjusted to a special group that is maladjusted to society as a whole. "The point of view here developed rejects all assumptions that would impute crime to the individual in the sense that a personal shortcoming of the offender is the cause of the unsocial behavior. The assumption that crime is caused by any sort of inferiority, physiological or psychological, is here completely and unequivocally repudiated." The author develops this thesis by showing how most criminals are simply grown-up delinquents;

how delinquents are members of gangs from which they derive a sense of escape, security, pleasure, and peace. The argument is dramatically augmented by accounts from the delinquents' own stories. Society handles the situation in the worst possible way by segregating, tagging, and dramatizing the delinquent until he becomes self-conscious and proud of the solidarity of the group of which he is a member. The author completes the picture by showing how our stupid forms of punishment reinforce criminal habits. Political corruption and inefficiency in the administration of justice are vividly presented by Morris Ploscowe, who contributes several chapters to this topic. The criminal emerges as a normal product of society, not as a monstrosity to be accounted for by some special psychological theory.

These three books, written from widely divergent points of view, have a certain basic relevance to each other. Fink throws the early theories of crime causation into perspective; from the vantage point of time he can afford to be critical of explanations once held as "true," now exhibited as outworn dogma. Dr. Pollens, writing of the current problem of the sex offender which he so glibly "explains" in terms of one theory, does not seem to be any less dogmatic than the early theorists; he evidently believes that the Freudian theory will have greater survival value. Professor Tannenbaum, swinging sharply away from the present trend toward individualization, questions the value of all these doctrines. He makes a strong case in favor of a consideration of the total social structure, but he does so by an almost exclusive consideration of the professional criminal. What he says would not seem to apply to the sex offender at all; nor to the first offender who was "a model child"; nor even to many of those who were convicted of murder or manslaughter under special psychological stresses. A study of the social background in such cases would give no clue to future events. But again, we shall have to wait for time to give us a new perspective.

EDWIN POWERS.

Cambridge-Somerville Youth Study.

COMMUNITY BACKGROUNDS OF EDUCATION. By Lloyd Allan Cook. New York: McGraw-Hill, 1938. Pp. xi+397.

SOCIAL PSYCHOLOGY OF EDUCATION. By A. O. Bowden and Irving R. Melbo. New York: McGraw-Hill, 1937. Pp. xv+296.

According to their authors, both of these books may be used as texts in educational sociology. Bowden and Melbo also suggest their book as a text in courses in the social psychology of education, and in educational psychology.

Community backgrounds of education is well written and enjoyable to read; the facts are well marshalled; the conclusions follow from the data, and the literature seems to be well covered. A similar statement cannot be made for *Social psychology of education*. Here one finds discursiveness, pages of vague generalizations, poor sentence construction, and a surprising lack of use of recent literature. The most recent citation is to an article published in 1935 (written by one of the authors); the next to a book published in 1931. Over half of the remaining forty-one citations are to publications from 1904 to 1924.

The book is replete with statements which are either poorly phrased, essentially meaningless, or outmoded by advances in psychological thought. A few quotations which are not far from typical follow:

> The human mind is a problem-solving machine (p. 25).
>
> The mind is an accumulation of past experiences . . . (p. 25).

They [emotions] are not vestigilial appendages, else long since they would have ceased to operate in everyday experience. This is true for the reason that nothing which is positively harmful can long survive in the individual (p. 26).

It [social psychology] is interested not so much in structural traits as in the neuro-psychic and neuromuscular mechanisms resulting and the social significance of behavior which proceeds from the structural conditions (p. 27).

In most cases it [homosexuality] points to death or personality annihilation (p. 118).

A group is extraordinarily credulous and subject to influence. It does not reason logically on the issue, but will believe a'most anything propounded to it and think in images which are very elementary (p. 224).

Some maladjustments are caused by a failure to react pleasantly to external environment, people, localities, and living conditions (p. 243).

Teachers sometimes fail because they develop an oppression psychosis (p. 250).

Although the book is concerned with topics of importance to the psychologist as well as to the teacher—attitudes and values, personality, school and classroom control, factors in administrative control, rumor, gossip, innuendo, propaganda, public opinion, and why teachers fail—it can be truthfully said that no topic is treated adequately.

The plan of *Community backgrounds of education* is as follows:

In brief outline, Part I of the volume presents a sociological overview of American community life. It describes three concrete communities, generalizes for each type, and concludes with a chapter on social planning for community life. Part II studies the school child as a community problem. Within the limits set by research materials, it discusses a number of child-shaping influences as to nature, personality effects, and implications for school progress. Part III directs attention to the teacher and the school in relation to the community. A concluding chapter summarizes a recent nation-wide survey of teacher attitudes and information, and suggests needed changes in teacher training (p. 14).

The communities dealt with are those studied by Mandel Sherman and Thomas R. Henry in *Hollow Folk*, by Albert Blumenthal in *Small Town Stuff*, by Robert and Helen Lynd in *Middletown*, and by Harvey Zorbaugh in *The Gold Coast and the Slums*. The psychological and social forces at work in each community are brought into relief. Of more direct interest to the social psychologist is the section on Social Forces Shaping the Child, where discussions of family, play and school life, as well as of child labor and boy and girl transients are concisely presented. An excellent introductory treatment of race relations is also included. Part III is of greater interest to teachers as such, the section entitled "Who Controls the School" and "Educating for Adjustment" being particularly valuable. This book well merits inclusion on lists of recommended reading for students of psychology.

ROBERT C. CHALLMAN.

Teachers College, Columbia University.

CHARACTER GROWTH EDUCATION. By Fritz Kunkel. (Translated by Barbara Keppel-Compton and Basil Druitt.) New York: Lippincott, 1938. Pp. xii+348.

EMOTION AND THE EDUCATIVE PROCESS. By D. A. Prescott and others. Washington: American Council on Education, 1938. Pp. xviii+323.

FOUNDATIONS OF EDUCATIONAL PSYCHOLOGY. By Peter Sandiford. New York: Longmans, Green, 1938. Pp. xiv+464.

In *Character growth education*, a book designed for teachers, parents, and clinicians, the author develops his views on "we-psychology" as it applies to the development of character from infancy to adolescence. It represents an

elaboration of the views expressed with telling insight in his *What it means to grow up*, published in 1936. It is puzzling to find that in referring to the "we-experience" in the blurb on the cover, the publishers state that "in this term the emphasis is of course laid just as strongly on the group as on the individual," whereas in the text the author remarks that "in this term, however, the emphasis is laid just as strongly on the individual as on the group!"

Essentially the "we-experience" relates to the fundamental importance of the necessity of social cohesion in the development of the uniqueness of personality. Without this feeling of "belonging," mutual exploitation arising from egocentrism and arrogance will result, although by developing an egocentric attitude we protect our feeling of superiority. One of the most apparent psychotherapeutic methods advocated by the author in the process of encouraging the transition from egocentricity to "we-ness" is that of "hermeneutics" by which the child's unconscious but real processes of thought and feeling are translated into a language he can understand. The "we-experience" is a reality to the individual even if he actually lives in social isolation. The mother-father-child relationships offer the logical and psychological basis for the healthy development of this growth toward maturity when the individual is able to extend the scope and increase the depth of human relationships. Objectivity is an attitude reflected in forms of behavior that appear to be the most adequate compromise between either too severe or too easy an environment. Extreme passivity and conformity as a result of over-indulgence or over-severe discipline restricts the chances of the adolescent or adult for living a rich, wholesome, and spiritual life. Hence the importance of early healthy parent-child relationships. The main task of education is to develop the "we-experience." In its broader yet essentially characteristic way the "we-experience" is a Christian one; through the fellowship of religious values the individual gains in self-reliance and strength.

The book is divided into three parts, the first dealing with "Early childhood," the second with "School period," and the third with "Adolescence." Part IV, concerned with "Child guidance," specifically refers to psychotherapy, a field in which the author has achieved considerable distinction. As far as can be seen, the main contributions the author makes in an attempted analysis of therapy with children are as follows: (1) the origins of individual unhappiness and maladjustment lie in the unhappiness and inadequacy of parent-child relationships tending to insecurity and frustration; (2) personal values and the understanding of others are enhanced by the presence of creativeness, sensitiveness, and faith; (3) religious values are emphasized as contributing to the creative possibilities of personality development.

In these times of changing ideologies this book offers a stimulating message to mental hygienists, educators, social workers, and parents. Perhaps now more than at any time in the present generation is there the necessity for incorporating within the framework of child therapy, case work, or the rearing of children the principles of "we-psychology"—despite the fact that they have not yet acquired the systematic status of the psychologies of Freud, Jung, and Adler. The statement that "the goal of education is to dispense with the educator" is heartily applauded!

Education and the educative process is a report of seven years' work by a group of eight authors in various fields of education, organized and sponsored by the American Council of Education. The panel was constituted as "The Committee on the Relation of Emotion to the Educative Process." The impor-

tance of this investigation cannot be over-stressed; although the author says "that the object of this report is to set down the findings of an exploratory study not to prepare a handbook for the new education" (p. 182), a reader interested in personality development or in education in general can hardly fail to appreciate its significance as a treatise on progressive education. It bases a worthwhile investigation upon the importance of affective phenomena in mental hygiene, the role of feeling and emotion in aesthetic experiences and artistic productions, the adaptation of curricula to conditions of mental hygiene, and the selection of certain areas of research and experimentation in school administration and practice where further investigation is badly needed.

The chapters vary in quality to a degree that might not be expected from the efforts of such a distinguished committee. Whether this is due to the scope of the problem surveyed or to the specialized interests of the committee members or to the organization of their respective abilities, the reviewer cannot say. A chapter dealing with basic affective phenomena offers nothing new as an interpretation and suffers from an attempt to survey and condense a vast field of research. As might be expected, considerable space is devoted to the genesis and development of specific and general attitudes and to their directive influence upon behavior and health. A portion of the book dealing with affective maturity raises several important questions with a formulation of the role education can play in that process whereby a person matures emotionally. Laudable attention is paid to problems of personnel in education. A final chapter reviews the general conclusions of the preceding portions of the text.

It is difficult to judge what professional group will chiefly benefit from reading this comprehensive report. More than any other class of readers, perhaps school teachers would gain most from reading the book carefully. Any well-informed clinical psychologist has little to gain from it. Students of personality development, aesthetics, and social psychology may find many possibilities for projecting their researches in the direction of public and private education.

Perhaps the section with most suggestions for further research is that entitled "Aspects of education needing study." Here, the reader's attention is directed to such topics as: the measurement of individual differences; the re-affirmation of the rights of the individual; the appropriateness of curricular experiences to the personality needs and abilities of children; the motivation of school tasks and general administrative considerations.

It is a pity that at times the authors' style is burdened with academic jargon and trite statements that make sections of the book dull and uninspiring reading. Despite this evident drawback, however, any serious student of education would do well to ponder the subject matter.

Foundations of educational psychology is a carefully planned revision of an earlier work that for many years was held in high esteem, although it was more widely known in British universities than in colleges in this country. The present volume is a scholarly treatise by an outstanding teacher, designed for advanced students who may wish to specialize after having studied the contents of this text. An indication of the stress placed upon an understanding of fundamental biological concepts is the fact that of the six chapters the one on "Heredity and environment" is the longest. In the scientific spirit and following a behavioristic tradition, the author gives considerable space to a balanced discussion of individual differences, nonvariable or unlearned activities,

and the foundations of behavior, such as physiological gradients, glandular and autonomic nervous system.

For a clear expression and adequate summary of an extensive field and a mature and sound evaluation of valuable information, the chapter dealing with "Intelligence—its nature and measurement" is outstanding.

From beginning to end of this book the author has maintained a clear concise style. In a text-drugged market this volume merits a prominent place in any well-chosen trio of texts of educational psychology.

P. S. DE Q. CABOT.

Cambridge-Somerville Youth Study,
Cambridge, Massachusetts.

READING, WRITING AND SPEECH PROBLEMS IN CHILDREN. By Samuel Torrey Orton. New York: W. W. Norton, 1937. Pp. 215.

Approaching the problem from the point of view of a neurologist, Dr. Orton presents in this book a theoretical and practical discussion of "certain types of disorders in the development of the language faculty." The book is a summary of the findings of a ten-year period of intensive study of language disorders in children and adults. Dr. Orton has found that in adults language disorders, consequent to brain injuries, occur only when the "master" half of the cerebrum has been injured. He postulates, therefore, that the development of one dominant hemisphere is normal and that lack of such dominance interferes with the language function. Thus, a child who inherits mixed laterality—right-handedness and left-eyedness, for example—is a child who cannot easily develop a "master" hemisphere. For such a child engrams are present in each hemisphere, one the mirror-image of the other. The normal elision of the engrams of the non-dominant area does not occur. At the "critical ages" of two to three and six to eight, therefore, the child is apt to be confused: he stutters, he reverses letters in reading and writing, he shifts spontaneously from left- to right-handedness. Similar symptoms may be induced by environmental factors. For example, social pressure toward right-handedness may prevent the left-handed and left-eyed child from developing a dominant right hemisphere. This, in brief, is the plight of the "motor intergrade," the child who because of inherited or environment factors fails to develop unilateral cerebral dominance.

A detailed description of six syndromes of delay or disorder in the acquisition of language is followed by a somewhat general review of the principles on which retraining is based. Specific methods of treatment employed on each syndrome are included. Dr. Orton presents these methods not for general use, but merely as suggestive of what may be done. He believes that strictly speaking each individual child presents an individual problem and that the remedial teacher should be trained to meet the specific needs of this one child. In his opinion remedial work with another child would necessitate retraining. Yet the techniques Dr. Orton describes are not only not peculiar to a neurological interpretation of reading and speech disorders, but are in general the tools of every astute remedial reading teacher. At best the theory in its present form is applicable to a very limited number of severe language disorders.

A glossary of technical terms used in the text is to be found at the back of the book.

THELMA G. ALPER.

Wellesley College.

AN EVALUATION OF VISUAL FACTORS IN READING. By Henry A. Imus, John W. M. Rothney, and Robert M. Bear. Hanover, New Hampshire: Dartmouth College Publications, 1938. Pp. 144.

For some years the Dartmouth Eye Institute has been making a study of ocular defects among Dartmouth College students, with special reference to aniseikonia. In 1936, an intensive investigation of all members of the class of 1940 was undertaken with the cooperation of the Eye Institute, the Department of Psychology, and the Administration. The present monograph is a preliminary report of the relationships found in this investigation among visual defects, reading ability and academic success. The authors also report on the effectiveness of their extensive remedial program.

Contrary to expectation, no significant relationship was found between ocular defects and performance on reading tests. In fact, the relatively few students with severe general ocular defects (57) or severe aniseikonia (20) scored higher on nearly every item in the test battery. The authors feel, however, that the number of cases involved is too small to permit of valid explanation. Again, not only was there no significant relationship between ocular defects and academic success, but also correction of ocular defects did not in itself guarantee either increased ability in reading or greater academic success.

A third significant finding was that photographic records of eye-movements are unreliable and invalid measures of reading ability. As a possible explanation for such lack of relationship, the authors suggest that the 50-word test passages provided by the American Optical Company for the college level are too short for diagnostic purposes and of a content too familiar to the college student. The authors recommend passages of at least 250 words. They fail, however, to note a further defect: namely, that the passages used are replete with dates and other figures and thus encourage "word for word" reading and frequent regressions even among good readers. Consequently these passages would not adequately differentiate between good and poor readers.

The monograph clearly establishes the value of remedial work at the college level even though no marked correlation between reading efficiency and academic success was found. The basic evaluation of any such program must be in terms of the gains to the individual as a total personality. The authors promise supplementary evidence on this point in their final and complete report.

The Dartmouth study is a significant contribution to the field of reading and to the more general problems of the college student. The program of investigation is an unusually ambitious one. Its value lies primarily in its very extensiveness, for it throws open innumerable questions for further study by ophthalmologists, educators, and psychologists.

THELMA G. ALPER.

Wellesley College.

THE STUDY OF SOCIETY. By F. C. Bartlett, M. Ginsberg, E. J. Lindgren, and R. H. Thouless. New York: Macmillan, 1939. Pp. xii+498.

The guiding principle of this book is a desire to make social science socially effective. The volume is the product of a series of meetings of a group of social scientists, comprising psychologists, sociologists, social anthropologists, psychopathologists, and others eager to mobilize their various tools and methods for the more efficient study of social problems. Such an aim cannot be too highly commended, for now is certainly the time when psychology must justify

its existence. We hope that this book will be the forerunner of other cooperative investigations by social scientists into important problems.

Of approximately twenty-five people who took part in the series of meetings, seventeen have contributed chapters to this book. We are told that the seventeen individuals did not write their contributions independently, but that instead each chapter was read and criticized by the group as a whole, so that the various chapters are more representative of the opinion of the group than they would otherwise have been. The seventeen contributors wrote a total of nineteen chapters, divided into four sections: problems of social psychology, social applications of psychological tests, methods of social anthropology, and methods of sociology. The book is addressed to research workers in the various fields and to amateurs who wish to contribute to the solution of pressing social issues.

In a book of this type, dealing with such a broad field, it is almost inevitable that each chapter will seem inadequate to readers familiar with that particular field. Such individuals will feel that a particular chapter neither gives sufficient detail to assist one who does know the field nor gives sufficient evaluation and discussion to guide one who is not familiar with the field. The second criticism is perhaps more telling than the first. Since the book is directed to the prospective research worker, many readers will feel that the chapters are too eclectic and too much in the style of a handbook to be of maximal value. The research worker, in addition to knowing the methods, must have sufficient critical discrimination to choose wisely among the methods and must know the precautions necessary with each method. In many ways, however, this book accomplishes these ends more efficiently than other similar books. Particularly do the sections devoted to social anthropology and sociology emphasize the conditions under which each method is applicable.

There is no way to review the content of this book other than to indicate the general nature of each chapter. In the first section of the book, dealing with problems of social psychology, there are five chapters. The first two suggest problems for research, the third suggests parallels between psychopathology and social psychology, the fourth reviews child psychology, and the fifth discusses terminology in social psychology. The second section deals with the social applications of various psychological methods. It includes a discussion of statistics, intelligence tests, questionnaires, attitude tests and rating scales, as well as of the work of the vocational adviser. The third section dealing with methods of social anthropology discusses various field-work methods and devotes a chapter each to the technique of interviewing and to the analysis of folk-lore. The fourth and last section, which discusses the methods of sociology, includes a chapter each on social case work, team work in social research, industrial social research, and social surveys.

The book is interesting, challenging, and informative. Certainly the basic aim of the book, to integrate the various methods of social science, is extremely worthwhile. It can be unhesitatingly recommended to anyone interested in making social science more valuable to society.

ALFRED L. BALDWIN.

Harvard University.

LOVE PROBLEMS OF ADOLESCENCE. By Oliver M. Butterfield. (Originally published by Bureau of Publications, Teachers College, Columbia University, New York, 1939.) New York: Emerson Books, Inc., 1939. Pp. viii+212.

In this book the author explicitly sets forth two primary objects of investigation. They are: "(1) To list the love problems of adolescence and to secure

some rough measure of their relative frequency. (2) To make some analysis and explanation of their relation to current culture pressures" (p. 181). The contents of the book, however, indicate that a third implicit object was also present; namely, to propose solutions to the problems which are listed. Unfortunately none of the problems is treated extensively enough to permit more than a superficial answer to the various questions that are raised. In general, then, two rather different trends of interest are apparent in the book. One is a socio-psychological analysis of adolescent adjustments, and the other is advice to adolescents and to their consultants. Since the latter trend is treated so inadequately (due to the breadth of the problems raised), the book suffers from the simultaneous pursuit of two diverging interests.

The material was gathered from twenty-nine groups of young people in Protestant churches, camps, and college organizations. The author, who acted as a consultant in these groups, recorded the questions which were raised. Most of the questions were ones submitted by the young people for group discussion. While this type of investigation is liable to pit-falls, the author has managed to obtain a remarkable amount of interesting material. The limited cultural backgrounds of his group are recognized quite clearly, and care is taken in restricting generalizations. Furthermore, the problem of frankness and genuineness on the part of the adolescents seems to have been solved with considerable success.

The questions obtained from these groups were assumed to represent the problems which were actually confronting the young people. In most cases this seemed to be a reasonable assumption. Then by reference to anthropological and sociological literature the problems were analyzed on a basis of the social situation from which they arose. The analysis is a functional one, with each problem related to its social context in order to understand the situation which created it. This type of analysis provides an opportunity for a better understanding of the nature of the adolescent love problems and produces some suggestive findings. The emphasis on cultural conditions, however, has been achieved at the expense of other variables. Although biological factors are mentioned occasionally, very little is done with them. The one-sidedness of the approach can be indicated by noting that there is not one reference to Freud's works in the otherwise extensive bibliography. Endocrinological factors, moreover, are dismissed with the assertion that too little is known about them for present consideration.

The book is divided into chapters which consider successively the various stages in the development from puberty to marriage. The chapters are titled: Starting Boy and Girl Friendships, Making a Good Impression, Keeping Steady Company, Engagement Problems, and Problems Concerning Marriage. Each chapter is divided into discussions of specific problems confronted at each stage of development. It is to be regretted that the attention given to each specific problem is so scanty. In the first chapter, for example, eight different problems are analyzed and remedies suggested, all in eighteen small pages. Throughout the book, however, reference is made to the literature in the field of adolescence, and a valuable bibliography with over 200 entries is included.

In general, the book is of value to persons interested in understanding the nature of adolescent adjustment because of the attention given to the situations out of which problems arise. Advisors, however, will find the proposed remedies of negligible assistance. The most convincing contention of the book is the repeated emphasis upon a need for more competent education in matters of sex and love.

In general, it may be concluded that while the study reveals no new problems, neither does it reveal any for which there is not considerable educational and psychological remedy. We live in an age of educational and factual abundance but we appear to suffer great privation from poor distribution of its resources. The hope for improvement lies in attacking the situation all along the line, with special stress put upon the training of parents (p. 189).

DORWIN CARTWRIGHT.

University of Iowa.

CURRENT PSYCHOLOGIES. By A. J. Levine. Cambridge: Sci-Art Publishers, 1940. Pp. 270.

The present volume is an attempt to bring some kind of new order into what the author regards as the chaos of modern schools of psychology. Levine seems to think that the task is achieved by noting the similarities among the following four schools: the Neurological School, the Gestaltist School, the Purposivist School, and the Freudian School. The synthesis is almost wholly at a verbal level, i.e., no attempt is made to discover community of meanings by tracing apparent verbal similarities back to initial data and observations.

It is difficult to know what audience the author thought he was addressing when he set out to write this book. If the book was intended for the layman, it is doubtful whether the layman will find much enlightenment, for the exposition offers no historical orientation, and is too sporadic and superficial to furnish systematic orientation. If the book was intended for students of psychology, the author has badly misjudged his audience, as the following excerpts, selected at random, will show.

The neurological school is intrinsically a synthesis of psychological principles garnered from various schools. . . . Architecturally, it may be envisaged as a structure composed of a central building fabricated out of the brick and mortar of isolated states of consciousness and innate capacities flanked by less substantially built wings in a state of constant remodeling. . . . It is mechanistic in its orientation. It assumes that simple states of consciousness combine and are elaborated into higher mental states. These form the content of consciousness. This is the "known" consciousness. There is also a "knowing" consciousness which supplies the energy and motivation for the "known" consciousness (pp. 21 f.).

The modern supporters of Aristotelian vitalism and entelechy, according to Levine, are Woodworth, Bühler, Orth, and Ach. These men showed that "the laws of association gave way to a teleological factor, the assumption of the presence of design in consciousness. The Gestaltist broadened the concept by including the notion of totality. Gestalt is a category of thought like causality and function. . . . The first step to their solution was made by Charles [*sic*] von Ehrenfels who pointed out that a melody remained the same when transposed in a different octave. . . . In the Gestalt theory, these conditions are summarized in the law of Prögnanz [*sic*]" (p. 77 f.).

C. C. PRATT.

Rutgers University.

AMERICAN PSYCHOLOGY BEFORE WILLIAM JAMES. By Jay Wharton Fay. New Brunswick, New Jersey: Rutgers University Press, 1939. Pp. x+240.

This book does not purport to make any new constributions to psychology, but rather to refute the popular contention, as voiced by Cattell, that "the history of psychology here prior to 1890 could be set forth as briefly as the alleged chapter on snakes in a certain natural history of Iceland—'There are

no snakes in Iceland.'" The volume is divided into a Prologue and three Parts. The Prologue summarizes briefly the psychological developments in America from 1640 to 1890 and the background upon which they took place. Part I is entitled "The period of theological and moral philosophy," and covers the years 1640–1776; Part II, "The period of intellectual philosophy," 1776–1861; and Part III, "The period of British and German influence," 1861–1890. The author points out that this is an arbitrary division, as there is naturally considerable overlapping.

Very wisely, since he is dealing with relatively unknown material, Fay devotes the major part of the book to quotations from the early American psychologists, allowing them to speak for themselves. This they do amazingly well. We find, for instance, that as early as 1752 Samuel Johnson discussed genetic psychology in his *Elementa Philosophica;* that Benjamin Rush, who died 77 years before James' *Principles* was published, anticipated the Freudians not only in the concept of phobias but also in their nomenclature; and that Thomas Upham, in 1840, recognized mental sets and introversion-extroversion, as well as anticipating to some extent the James-Lange theory of emotions.

Of course, as Fay points out, despite a number of such precocious spurts, this so-called early psychology in America was quite unlike that of today. Such as did appear was heavily veiled in theology, scholasticism, and philosophy. Nor can it be said that the reading of this volume contributes substantially to one's grasp of psychology as it is now understood. Its value is purely as an historical document: the contributions of these men are for the most part of scant significance in the long-run history of psychological development, and in general do little more than reflect the influence of various European schools of thought. Yet, whatever the ultimate value of their efforts, these efforts should be noted in order to give a true picture of the substructure upon which modern psychology in America was built. For this reason, if for no other, Fay deserves commendation for his enlightening book on this period.

M. ELEANOR PIERCE.

New Haven, Connecticut.

A HISTORY OF ESTHETICS. By Katherine Everett Gilbert and Helmut Kuhn. New York: Macmillan, 1939. Pp. xi+582.

Although this work purports to be a history of esthetics, it virtually reaches its climax in 19th-century German idealism and curtails the treatment of more recent thought. As a result the book will seem to some, above everything else, a testimony to the divergence of point of view and method between the philosopher on the one hand and the psychologist and sociologist on the other. In fact, the latter type of inquiry is charged by the authors with having precipitated a crisis in esthetic thought. Recent estheticians, among whom psychologists hold a prominent place, have produced, according to this allegation, an unfortunate "departmentalization of esthetic thought," because they are "specialists, prompted by some particular knowledge and a general ignorance," which do not contribute to the advancement of the field.

Because of this intellectual temper of the authors, they have pressed the last ounce of erudition out of Plato, Aristotle, Baumgarten and Hegel. But Dewey and Lalo are given two lines; experimentalism is limited to Fechner and Helmholtz; psycho-analysis and the Marxist schools, both of which have wielded some influence in creative art and critical esthetics, are not indicated. With

practically all of the psychological and sociological theories post-dating Herbart, Fechner and Taine, workers in these fields will therefore hardly be content with the critiques of the more recent researchers as offered in this volume.

With this reservation on point of view—which undoubtedly others would consider a virtue—the book has pedagogical merits. The academic impartiality of much of the discussion, the clarity of style in often abstruse materials, the marginal guides, and many other qualities will commend it as a text to those who are of the philosophical persuasion. It is a handy reference for those who are interested in a reasonably complete sketch of "pre-scientific" esthetics. It satisfies a need which no one has attempted to fulfill on such an ambitious scale since Bosanquet released his memorable volume at the turn of the century.

J. H. MUELLER.

Indiana University.

CHILD PSYCHOLOGY FOR PROFESSIONAL WORKERS. By Florence Teagarden. New York: Prentice-Hall, 1940. Pp. 641.

When an author writes for a restricted field, it sometimes happens that the book is not useful to anyone beyond the group for whom it was written. This is not the case with Teagarden's *Child Psychology*. Anyone who is interested in the biological phases of child development and in such problems as social placement, will find excellent material in the first three chapters and in Chapters 7 to 9. The first three chapters deal with heredity and the principles of heredity as applied to the case load, and with all of the material which has to do with pregnancy, birth, naming the child, hospitalization versus home delivery, and so on. Chapters 7 through 9 deal with problems arising with the child in his home and away from his home, with adoption and many of the legal aspects of child care and placement. These chapters are not only of interest to social workers; they would make excellent reference material for any student of child psychology. The last chapters deal with the handicapped child. As can be seen from a brief review of the material presented, the book is not a psychology, but it contains much material of interest to psychologists. It is indeed one of those books which should be on every reference shelf.

ADA HART ARLITT.

University of Cincinnati.

COMMON NEUROSES OF CHILDREN AND ADULTS. By O. S. English and G. H. J. Pearson. New York: Norton, 1937.

THE TROUBLED MIND. By C. S. Bluemel. Baltimore: Williams & Wilkins, 1938.

PSYCHOPATHIC STATES. By D. K. Henderson. New York: Norton, 1939.

The social importance of psychopathic personality receives recognition in all three of these books. In the first two, it is only one of the many problems touched on; in the third it is Dr. Henderson's central thesis.

Dr. Henderson finds that "the psychopath is the most disruptive element in society. The delinquent, insane, and mentally defective are cared for under institutional conditions, whereas the psychopath is sufficiently well to be at large, because as yet there is no adequate provision for him." Psychopaths "do not benefit from prison treatment," nor indeed from any punitive efforts.

Dr. Henderson's purpose in this book is to remedy the laissez-faire attitude, commonly adopted among psychiatrists. He deplores the fact that most analyses of psychopathic states are "on a descriptive basis, and give little or no hint of

how psychopathic states are caused." He insists that "we are not dealing with static peculiarities of a constitutional nature; but that we are face to face with dynamic processes which are much more modifiable than has been supposed." Hence, "a needlessly fatalistic attitude has arisen." He contends that "no type of case, either for the sake of the individual or of society, is more in need of specialized help and guidance."

The laissez-faire attitude of which Dr. Henderson complains is well exemplified in Dr. Bluemel's book, which seeks to describe all the common symptoms of psychoneuroses, psychoses, and various other abnormal conditions for medical students and laymen. The very comprehensiveness of Dr. Bluemel's undertaking makes superficiality inevitable. He cites instances of successful treatment for other kinds of patients but not for psychopaths. He brands psychopathic personality as "constitutional infantilism." He draws a needless and misleading distinction between this and "acquired infantilism," which he thinks results from "lack of discipline." He cites a few instances of the latter. But he finds that many individuals whose early life showed no lack of discipline grow up thoroughly selfish and ruthless. He points out that "punishment is a poor deterrent for the psychopath." Since the kind of discipline in which he believes is powerless to effect reform, Dr. Bluemel jumps to the conclusion that "the psychopath" is a "biological failure." Although he does not make it clear what he means by "biological," the context implies that it has nothing to do with training or early experience. If not identical with heredity, it has some mystical significance that absolves the parent, the educator, and the psychiatrist alike.

Dr. Henderson, Dr. English, and Dr. Pearson seek no such easy alibi. Dr. English and Dr. Pearson point out that the terms "constitutional psychopath," "constitutional psychopathic inferior," "moral insanity," and "psychopathic personality" were meant to convey "the inherited etiological factor which was supposed to produce them." These two psychoanalysts prefer the term "character neurosis." They find the roots of this, as of all other neuroses, in early childhood influences. Unlike Dr. Bluemel, they do not stress lack of discipline. They find lack of love a more frequent and common cause of rebellious behavior. Even conspicuous cases of overindulgence often mask parental rejection, which the child recognizes and resents. Criminal tendencies represent fixations of libido at infantile levels, commonly because the infant was too much frustrated.

Dr. Henderson likewise points out "how large a part frustration may play in creating habit responses of a terrifying and alarming nature, and when restriction is removed and free scope is given for individual expression how much benefit occurs. . . . We see such reactions at all stages of life in the nursery, the school, the university, the factory and wherever liberty is curtailed to an extent that proves intolerable. Society, instead of attempting to understand the situation, is apt to deal with it by increasing restriction and almost inevitably greater turbulence is produced."

When we read of some of the treatments that Dr. Bluemel advocates in all seriousness for various obsessions and minor psychoneurotic symptoms, we are not surprised that he gives up the psychopath as a "biological failure."

Dr. Henderson likewise maintains a biological orientation, but for him the term "biological" is no mystical alibi. It includes heredity, but it also takes account of every change that may have been wrought by environment and training. He says "delinquency is not so much due to our innate evil disposi-

tion, but rather is determined by our psychobiological development in the light of our individual life-history."

Whereas Dr. Bluemel's book gives a graphic description of symptoms with a dearth of intelligible etiology, the etiology is beautifully clear in every case cited by Dr. English and Dr. Pearson. If anything, the picture is almost too pretty. Certainly the medical student who has time for only one book on the psychoneuroses had better read English and Pearson rather than Bluemel. From the former he will at least learn to look for intelligible causes and to avoid superficial symptomatic treatment. His chief hazard will be the naive acceptance of psychoanalysis to the exclusion of every other approach.

Dr. Henderson is of these authors the most broad-minded. He recognizes the significant contributions of psychoanalysis, but he says "in our evaluation of all the circumstances, etiological and otherwise, it is imperative that we should pay as much attention to the conscious and obvious as to those symbolic acts and thoughts which make us speculate in terms of unconscious motivation and determination. . . . The analyst pays little or no attention to the conscious and obvious situations which are often of so great importance. This one-sided approach is a dangerous type of medical practice." "Psychobiology," says Dr. Henderson, "does not oppose any method of treatment . . ., but it is the basis for the application of all the specialized methods of our psychiatric armamentarium and is their common denominator and coordinator."

Dr. Henderson points out that the psychopath's "condition has, as a rule, developed insidiously throughout a period of years, and for his own betterment and for the protection of society, his efforts toward readjustment demand supervision, but under such elastic conditions that restriction and confinement do not become too irksome."

Both Dr. Henderson and Dr. Bluemel give considerable attention to the psychopath who attains a position of leadership. Dr. Bluemel calls him "the public lunatic." Dr. Henderson calls him "the genius." Dr. Bluemel says, "the psychopath is active in religion, in crime, in business, in politics; indeed he is an ubiquitous disturber of the peace." Dr. Henderson says, "the genius must always be a law unto himself." Dr. Henderson thinks of "genius as associated with a state of mental imbalance, of heightened sensitivity, of disordered mental equilibrium due probably to the attempt to get square with reality and even more to dominate reality as a compensation for the inner unresolved conflicts which dominate conduct."

Both psychiatrists comment on military leaders in their discussion of psychopaths. Says Dr. Bluemel, "Warfare is as futile as a quarrel between two paretics, yet it is the method by which political madmen settle their differences." When the common citizen "encounters an idiot or a paranoiac, he places him behind institutional walls and deprives him of his pocket-knife. Meanwhile he ignores the public lunatic and permits him to retain his military armaments."

MABEL F. MARTIN.

Northampton State Hospital,
Northampton, Massachusetts.

IST DIE AGRESSIVITAET EIN UEBEL? By Tora Sandstroem. Stockholm: Albert Bonniers, 1939. Pp. 187.

The author of this volume calls his book a new orientation of psychoanalysis. And the attempt seems to me one of the best critical appraisals of Freud's

analytical conception I know. The main thesis is opposed to the overestimation of the role of sex in mental life, a point on which others too—for example, Carl Gustav Jung—have based their opposition to Freud. For Sandstroem, as for Jung, the point of departure is a dynamical conception of the human soul, and for both writers the background of the most frequent neuroses is a weakness of the ego or self. The principal issue of the new theory of Dr. Sandstroem is a positive conception of what is called "aggressivity," hitherto commonly regarded as a negative social activity. In this book aggressivity is taken as a positive propensive power to live, to execute, and to carry through—a conception recalling somewhat the philosophy of Max Stirner. The contradiction of weakness and psychic strength comes near to Janet's conception of the "force et faiblesse mentale." Very original, however, is Dr. Sandstroem's theory of a higher or collective ego "Ueber-Ich," which is a social and categorical factor of great weakening influence and which replaces Freud's "It" or "Es." Likewise important is the therapeutical application of that idea, going far beyond the same dualistic conception of Janet. In addition, Sandstroem's application of the theory to social life is most interesting. He says, for instance: "Restricted in his aggressivity an individual often identifies himself with his suppressed affects and becomes a communist, or he identifies himself with his too strong *Ueber-Ich* and becomes a fascist."

ERNST HARMS.

New York City.

RELATIONSHIPS BETWEEN YOUNG SISTERS AS REVEALED IN THEIR OVERT RESPONSES. By M. B. McFarland. New York: Bureau of Publications, Teachers College, Columbia University, 1938. Pp. xi+230.

This book reports a study of 22 pairs of sisters under seven years of age. Each pair was observed three times in uncontrolled situations and twice in situations using certain play materials.

The records (made by an observer in the room) were analyzed according to the following categories: social interaction; cooperative activity; sharing; conflict; physical play; imaginative play; parallel activity; direction; submission; resistance; unsolicited following and imitation; rivalry; helping; protection; sympathy; affection.

The author stresses the variation in responses, coupled with "trends toward certain types of response" so that an individual pattern of behavior was observable. The trends are interpreted as indicating basic attitudes toward the sister.

The author comments on the support this study gives to an Adlerian point of view, but attempts no theoretical conclusions.

DONALD W. FISKE.

Harvard University.

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